Systematic Zoology

SOCIETY OF SYSTEMATIC ZOOLOGY

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Systematic Zoology

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Points of View

The Promotion of Research

That research in the sciences is in itself good; that it is to the national advantage as well as to the advantage of the human race as a whole to strengthen the activities of research; and that research in pure science is more in need of support than research in the applications of science or in the fields of science that most lend themselves to the furtherance of industry or public health seem to be propositions widely agreed upon. Attention is now focussed on the problems of the promotion of research in the United States by the establishment of the National Science Foundation by the federal government specifically for this purpose.

The national promotion of research had become a question and problem in Great Britain in the eighteen-eighties, in the face of the pre-eminence of Germany and of German universities in several fields of science. One of the greatest biologists of that day, Sir E. Ray Lankester, offered the succinct advice that research in general is best promoted by the establishment of salaried positions that ensure continuity of research and the independence of a senior researcher; further continuity to be provided by the establishment of long-term positions for one or more assistants. Efficiency of senior researcher and his aids requires clerical and laboratory assistance and finally, financial aid to ensure the publication of results.

Lankester's advice is as sound in 1952 as it was in 1885. It is crucial to the effective strengthening of research activities to provide for *continuity*. Many research programs require long-term plans and long-term operation; and short-term researches need to be brought into relation with larger over-all plans. The basic recommendation for the efficient disbursement of funds for research is that they be used for the strengthening of *existing* institutions, for the expansion of their staffs and for the strengthening of existing scientific journals. The establishment in some fields of science and in some parts of the country of new research institutes on a permanent basis should be envisaged where an especial weakness or an especial need is discerned.

Currently, there has been much effort, the value of which I by no means discount, to promote research by means of individual short-term grants—fellowships, especially for students at the post-graduate level, and support of individual projects. This produces the basic difficulty that research workers are trained for whom no lifetime research career is possible. Research is packaged and parcelled into individual projects, which remain unrelated unless they can be continued or integrated into long-term research. The main outlet for research men trained in universities is still teaching, and general teaching at that. If our researcher is a good teacher, he will in turn produce more research-trained men than can be supported by existing institutions. Research methods and procedures and above all the inspiration to carry on research are even more effectively transmitted to apprentices than to classes of students.

Research, I believe, would be more effectively supported by the strengthening of existing institutions than by the fellowship and grant-in-aid method. This would very often mean the supply of aid to established research men and women, to whom secretarial help may be even more vital than laboratory assistance, and who should not accept the aid supplied by subsidized fellowships unless addi-

tional secretarial and clerical aid is supplied. In brief, research might often be more effectively fostered by the provision of a permanent secretary than by the training of an additional research worker. Beyond this, existing institutions need expansion to cover their stated fields more adequately by the addition of permanent staff members. In most museums it is actually only the vertebrate subphylum that is adequately staffed. A survey of the research institutes (or departments of museums and universities that function as research institutes) would bring into sharp relief the weakness of support of systematic zoology.

The fields of biology for which I can speak with most authority are systematic botany and zoology. These sciences form the foundations of biology. The systematic classification supplies the framework according to which studies of the natural history of living things can be arranged so that even trivial observations may find their ultimate place in a harmonious structure. These foundations of biology are in need of constant inspection, repair, rebuilding and extension, quite as are those of a skyscraper; and it is pertinent that the understructure of a skyscraper must be so vast and complex as to compare with the extent of the superstructure. After two generations of relative neglect on the part of university biologists, in which the remaining systematists (especially those in the research institutes maintained by museums) have carried on as best they could, it becomes evident that the classification of plants and animals is in need of a comprehensive re-study, partly because the work of the first period of description was in the nature of a first approximation, still more because the whole classification needs to be re-examined in the light of the newer biological sciences, such as physiology and genetics.

The relative neglect of systematics by the university centers of biological research has had the particularly unfortunate result of diverting competent students from systematics into the supposedly more advanced and important fields. This requires mention because a competent systematist can not be trained in a few years of graduate research work at a university. Competence in a systematic field, and thus usefulness to general biology, is not to be gained short of half a lifetime of devoted effort. Thus it is particularly disturbing to find whole phyla of the animal kingdom, especially among marine invertebrates, with no authoritative specialists in the western hemisphere.

With the rise of the synthetic science of *ecology*, which pieces together elements from all of the sciences to gain new insights at a higher level, the need for sound elementary systematics becomes acute. Nothing short of the establishment of new institutes for entomology and invertebrate zoology generally can fill out the growing weakness in these fields of zoology.

Any expansion of effective research activities will involve a corresponding increase in published results of research. Most scientific journals, including the scientific publications of museums, are already swamped with an oversupply of manuscripts, and there is above all a need for journals that can publish longer papers (which sometimes may mean the more important ones) and especially for the synthetic and summarizing works that form the springboards for renewed plunging into research. It would be reasonable to suggest that the National Science Foundation allocate at least ten percent of all funds to a publication fund used to support existing journals and the societies that bear the burden of publication in their respective fields.

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On Uniform Group Endings

In his judicious and forceful discussion, "For and Against Uniform Endings in Zoological Nomenclature," in the opening

number of SYSTEMATIC ZOOLOGY (pp. 20-23), George Gaylord Simpson pays particular attention to the recent proposal by H. B. Stenzel (*Science, 112,* 94, 1950) that nine uniform name endings be adopted for ordinal (-ica, -ida, -ina), familial (-icae, -idae, -inae), and tribal (-ici, -idi, -ini) groups. This scheme has also been criticized by Alvarez (*Ciencia, 11,* 279-280, 1952).

Stenzel's proposal is so alluring in its simplicity, consistency, and logic as to inspire, on first thought, agreement and adoption. It received support, for example, from the American Society of Ichthyologists and Herpetologists at its 1951 meeting, particularly from the ichthyologists present, the writer included (the herpetologists were particularly opposed to the abandonment of their relatively few and well-known ordinal names).

On more thoughtful and analytical consideration, Simpson points out how confusion and even error might well arise from the acceptance, for groups of different rank, of names so very similar as Hominica, Hominida, Hominina, Hominicae, Hominidae, Homininae, Hominici, Hominidi, and Hominini. I would like to bring to the attention of systematic zoologists a more serious defect of Stenzel's proposal, namely, that it would deprive us of the use, now frequent and desirable, of definitive group names in adjectival and derived substantival form, or would throw such usage into hopeless confusion. Thus, if we should write about "poeciliid" fishes (or gonopodia, or life-history), or of the distribution, genetics, or anatomy of the "poeciliids," it could not be known, under the system advocated by Stenzel, whether reference was to the order Poeciliida, the family Poeciliidae, or the tribe Poeciliidi. Under the current system we write about cyprinodonts (or poecilioids, or microcyprines), poeciliids, and poeciliins, when referring respectively to order, family and tribe (the subfamily adjective is poeciliine).

It would, of course, be possible to devise a system of uniform group-name endings, through at least the ordinal series, that would be less confusingly alike and would permit the derivation of distinctive adjectives (and of the corresponding vernacular substantives). Objections raised specifically to the system of Stenzel could thus be avoided. Other objections, such as that of supplanting literature-filled and familiar group names, would remain. An additional and fundamental disadvantage of the Stenzel proposal, or of any similar system of boxing the zoological compass, seems to me to be the inherent emphasis on group ranking, which I regard as a particularly subjective, superficial, and inconsequential (though perhaps not altogether unavoidable) aspect of taxonomic work.

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This note is Contribution from the Scripps Institution of Oceanography, New Series, No. 596.

Editor,
SYSTEMATIC ZOOLOGY:

First of all, let me congratulate you and your colleagues on the establishment of a new journal for systematists and taxonomists. It is indeed encouraging to see the science once again achieve its proper position in the field of biology. I trust that the journal will have the effect of leading all biologists to the realization of the fundamental importance of taxonomy in the study of organisms of all sorts.

I should like to comment, as a taxonomist, not of animals, but of plants, upon one of the articles of Volume 1, number 1, which seems to be not in keeping with the spirit of the times, although expressed by one whose commitments in the field of change are accepted without reservation by a good share of the world's biologists, this writer included. The article to which I refer is by Dr. Simpson, "For and Against Uniform Endings in Zoological Nomenclature."

The title itself causes one to raise his eyebrows and ask: "Is there still some

argument as to the value of such?" To be sure, I do not have at my finger tips all of the knowledge necessary to know one or more orders, classes, or even phyla of animals, nor do I think it necessary even to try to remember which one is being referred to in some particular instance. I cannot remember, for example, whether Coleoptera is a phylum or class, no more than I can remember whether Diptera is a class or a family. Uniform endings would help very much for say, a physiologist, to know of the size of the group with which a particular author deals. To be sure, I can refer to an unabridged dictionary for such information, but why should this be necessary?

I think an example from the field of botanical taxonomy is worth inserting. A South Australian taxonomist publishes a work dealing with a little known group of plants under the supposed title "A Delimitation of Himantandraceae." I know immediately that he speaks of a group of family level, and is not speaking of genus, or order, or class, or phylum. I am fortunate not to have to read either the first paragraph, or the summary to find out whether it is worth my while to consider the work.

Dr. Simpson's argument against changing the prevailing confusion is that it would require a lot of work to reorganize, to cull out the bad, to keep people who are seekers after publicity from putting many useless names before us. These are noteworthy arguments, and ones which should be thought of, but is this enough of an argument? I venture to say that most of the contemporaries with Linnaeus proposed much the same sort of argument against a binomial system.

I have not referred to the value of such change as it would affect the efficiency of learning for students who are beginning in the field, but the arguments in favor of uniformity seem to far outweigh the arguments against. First of all, a student may remember the word "order" but will he remember whether this goes with a word of class value or the proper taxon? Second, the student is told that taxonomy is the place where biologists keep their house in order, but what sort of order is it when he can't find uniformity even in the names?

With these few words, I cast my vote with H. B. Stenzel—"for," not "against" uniformity.

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Hybridization in the Purple Grackle, *Quiscalus Quiscula*

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WHEN natural hybridization in animals is discussed, the example of *Quiscalus quiscula* is usually mentioned. Many examples of such hybridization are known, but the grackles are unusual in that there is a considerable zone in which the intermediates are found in abundance without either of the end forms. Chapman (1936, 1940, and other papers) accounted for the present distribution of the species on the basis of a post-glacial reunion between populations isolated in Florida on the one hand and southern Texas and northeastern Mexico on the other. This theory was based on studies of the geographical distribution of the brilliant and often iridescent interference colors of museum specimens of male birds presumed to be breeding. The differences between the forms are seen chiefly in the feathers of the back. In the bronzed grackle, *Quiscalus quiscula versicolor,* these feathers are uniformly brassy-bronze to olive-bronze. The range of this form extends north to the limit of trees and west to the Rocky Mountains from a line running from southwestern Louisiana north through Mississippi and east through southern Tennessee to the Appalachians, then northeast across the mountains, reaching the Atlantic coast in the region of New York City. The other well-defined end form is the Florida grackle, *Q. q. quiscula,* whose dorsal feathers are dark green on the exposed terminal part, bordered basally by an iridescent band. This form is found south of a line running from southern Louisiana across southern Mississippi and Alabama, central Georgia, and western South and North Carolina, reaching the coast in the region of the Virginia capes. The specimens of these two forms examined in this work came from the localities shown in Map 1 (p. 168). Overlapping these ranges and occupying the region in between are intermediates completely connecting the end forms. Those which have brassy-green backs and merge into the bronze-backed *versicolor* are called *Quiscalus quiscula ridgwayi,* and the purple-backed intermediates which blend with *quiscula* are called *Q. q. stonei.* Map 2 (p. 169) shows the distribution of specimens of these forms and their intermediates with each other and with the end forms.

The color of the head and neck of this species varies from purplish-brassy through blue to brassy-green, but this variation is largely independent of that of the back color. The whole range of head colors has been observed in association with each of the back colors, but green and blue heads are not commonly associated with the dark green back. In Florida the head is always purple, but blue and green heads begin to appear north of that state. The relative frequencies of the head colors do not differ appreciably from *stonei* through *ridgwayi* to *versicolor.*

Chapman proposed that during one of the glacial ages, probably the most recent one, the Wisconsin, the two end forms became differentiated from each other while in their refuges. In the northern part of the range of *quiscula,* the green-backed Florida form, a purple-backed form, *stonei,* became differentiated. As the climate warmed up, *stonei* met and interbred with *versicolor* along a line from Louisiana to Massachusetts, producing a brassy-green-backed hybrid form, *ridgwayi.* Chapman suggested that the purple

form originated as a mutant similar to a purple-backed bird taken at Pine Island, Florida, on February 10, 1888, but offered no explanation of how this character might have become fixed in a large population of birds in the south Atlantic and Gulf Coast states.

In undertaking the present work it was hoped that a promising lead might be found for studying ecological factors which might account for the distribution of the species; but since it was doubtful whether such a basis for study could be found and followed up, three other approaches to this problem were used. These approaches, it was believed, would best utilize the available resources to answer some of the questions posed by the distribution of the species. The first approach was the statistical analysis of measurements of series of museum specimens, especially intermediates, in the hope of finding recognizable distribution patterns in characteristics which could be precisely measured and quantitatively evaluated. The second approach was the study of the behavior of individual birds in a mixed breeding colony, identified by colored leg bands, in an attempt to learn more about the behavior of the different forms towards each other. The third approach was the examination of the banding records for this species in search of evidence on mixing due to migration and hints which the present migration routes might yield as to the former invasion routes.

Ecological Variation

If there is an incipient ecological differentiation between *versicolor* and *quiscula* or between *versicolor* and *stonei*, the birds are so versatile in their adaptability that it seems hopeless to look for any widespread consistent differentiation of this kind. The most likely place to look for a contrast in habitats is Louisiana, where *versicolor* and *quiscula* come closest together. According to McIlhenny (Postscript to: Chapman, 1936), *versicolor* nests in the more hilly, drier land of northern Louisiana, timbered with longleaf, loblolly, and slash pines and red oak, while *stonei* nests in Spanish moss clumps on the cypress and tupelo trees growing in or near the water. *Quiscula* is largely confined to the coastal marsh-grass region of the delta in the southeastern part of the state (U.S.D.A., 1924), where it is accompanied by *stonei*. There is even one report (Beyer, 1899) of the occurrence of *versicolor* and *quiscula* breeding in the same gum swamps north of Lake Pontchartrain. East of Louisiana this correspondence with the vegetation and terrain does not hold true. Along the Atlantic coast the range of *quiscula* coincides roughly with that of the southern pine forest, while *stonei* and *ridgwayi* are the typical birds of the piedmont forest region, and *versicolor* begins more or less with the oak-chestnut forest of the central mixed deciduous region. These distributional relationships indicate that if there is any ecological differentiation with respect to the prevailing vegetation, it changes from one part of the country to another.

Geographical variation in habitat preference is quite evident when we consider the behavior of this species with regard to human habitations in various parts of the range, but the variation does not seem to be related to the distribution of the different forms. Indeed, the species as a whole seems to have a great ability to adapt itself to the changed conditions resulting from the clearing and settling of the land. Forbush (1927) believes that the species is "normally" a bird of the marshes and water-side in forested regions. In Newfoundland the bird is not well known, because it shuns the settlements, preferring the inaccessible thickets and boggy woods (Peters and Burleigh, 1951). This is in striking contrast to southern New England, where the birds nearly always nest close to houses and even make themselves at home in parks in the largest cities. The evidence (Huntington, 1952) seems to indicate that, with

the opening up of the land these birds have not added the cultivated areas to their former habitat, but rather have actually abandoned the woodland in favor of the more open country around farms and in parks and suburban areas.

Since the food supply is such an important factor in the choice of a habitat, it might be expected that the food habits of this species would show no more indication of differentiation between the races than the habitat does, and this seems indeed to be the case. The omnivorous nature of the grackles is attested by many writers (Beal, 1900; Hamilton, 1951), but very few of them present any quantitative information permitting comparisons between different color forms or different regions. Despite the great variety of foods taken and of methods of taking it, one general tendency does appear, although it is not supported by much quantitative evidence. Birds which remain in Florida and along the Gulf and southern Atlantic coasts would have soft foods such as insects and fruit available in larger quantities throughout the year than those which winter farther north. One would therefore expect the diet of the latter to contain a larger proportion of oak and beech mast, grain, and other seeds. The evidence on this point is indirect but of considerable interest. A number of genera of the Icteridae have a horny knob projecting down from the palate. In *Quiscalus quiscula* this is developed into a sharp ridge, truncated anteriorly. Beecher (1951) points out that this sharp cutting tool, brought to bear against a hard seed by the powerful adductor muscles of the grackle's jaws, has the effect of a short powerful finch-like bill without sacrificing the advantages of a long bill. Wetmore (1919) observed that this keel was well worn in old adults of *stonei, ridgwayi,* and *versicolor,* but that similar specimens of *quiscula* showed little or no wear. He reported that a small sample of *quiscula* stomachs showed a preponderance of insects and fruits, with very little mast or grain. The keel is less used in *quiscula,* although apparently just as well developed, indicating an absence of genetic differentiation in this character. The general shape of the bill, however, does seem to show genetic differentiation, being slightly longer and slenderer in the south, apparently an adaptation to the variation in diet. In spite of the fact that this difference in the bill appears to be genetically determined, we can not be certain that the difference in diet which has apparently favored selection for the bill difference is anything more than another manifestation of the euryecous nature of the species; that is, the difference in diet may be non-genetic even though it influences selection affecting genetic characters (Cushing, 1944).

The nesting site seemed more promising as a source of significant geographical variation than either the habitat or the food habits, since it is not subject to seasonal variation. In general, the species prefers to nest in conifers, but a great variety of other sites is used when coniferous accommodations are not available, and sometimes even when they are (Forbush, 1927, and others). These other sites include cavities as well as exposed sites. Of 157 passerine species found in New England (Forbush, 1927; 1929), 15 nest exclusively in cavities, and 13 are known to nest both in cavities and in the open. Most of those which nest in both types of site are very common birds, and in the case of the grackles this behavior fits in so well with their euryecous nature and has been so repeatedly observed that there is little doubt that it has helped them to become so successful. Hole-nesting is not confined to any one region; apparently holes are marginal sites, used when more desirable ones are not available.

This consideration of general habitat, food, and nesting site yields the conclusion that variation in none of these is correlated with color variation. Because individuals of the species are so adaptable, the species

TABLE I—REGRESSION OF WING LENGTH ON LATITUDE

VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	VARIANCE RATIO	PROBABILITY
Slope	1	3,457.41	3,457.41	112.47	<0.001
Scatter of means	21	583.61	27.79	. 0.90	>0.2
Deviations about means (Error)	645	19,829.06	30.74		
Total	667	23,870.08			

has been able to occupy a widely varying and rapidly changing environment with very little genetic modification. Without the divisive pressure of genetic adaptation to different environments, it has retained its identity as a single species with only minor subspecific differentiation in the face of circumstances which apparently threatened to split it into two species.

Analyses of Wing and Bill Length

Preliminary observations indicated that northern birds of this species had longer wings and shorter bills than southern ones. To test for the significance of the regression on latitude of the linear measurements of about 700 male museum-specimens taken in May or June (except for birds taken in Florida, where the species is non-migratory), the regression coefficient was calculated and an analysis of variance was set up as demonstrated by Bliss (1951, 460 ff.). The analysis of variance for the wing length is shown in Table I.

The northward increase in wing length is quite regular, as is shown by the fact that the scatter of the means of the degree groups relative to the variance within the groups is not significant (Fig. 1).

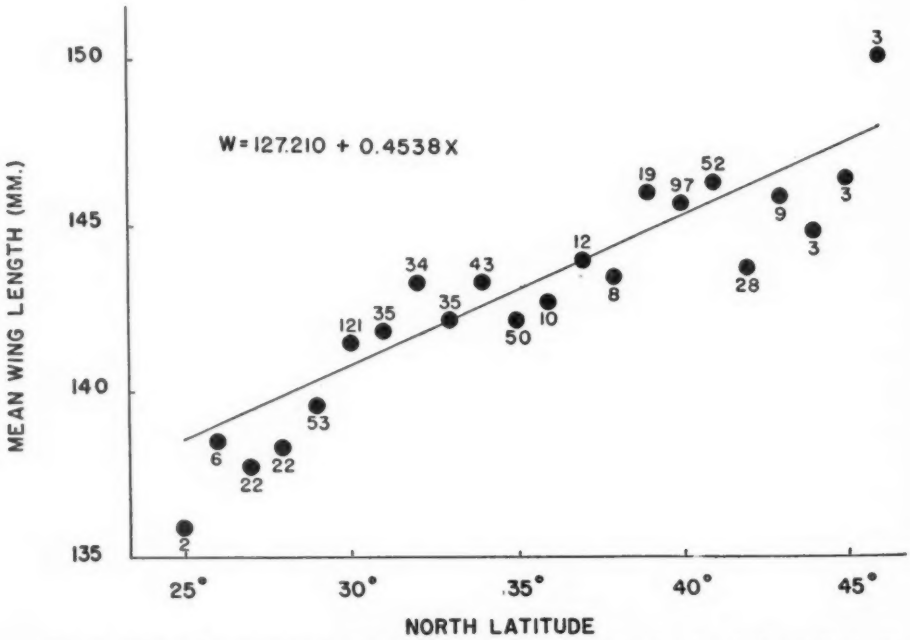


FIG. 1. Regression of wing length on latitude. The numbers by the circles indicate the number of specimens in each degree group.

TABLE II—REGRESSION OF CULMEN LENGTH ON LATITUDE

VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	VARIANCE RATIO	PROBABILITY
Slope	1	152.16	152.16	86.80	<0.001
Scatter of means	23	161.61	7.03	4.01	<0.001
Deviations about means (Error)	638	1,118.35	1.75		
Total	662	1,432.12			

In many species of birds the wing length is proportional to body size in general. The few available data on body weights in grackles give no reason to believe that this species is an exception in this respect. Amadon (1944) reports the mean weights of 8 male Florida grackles and 23 male bronzed grackles from Ann Arbor, Michigan, all taken in the spring. These means were 108.1 and 120.2 gm., respectively. The mean wing lengths of 31 male Florida grackles and 38 male bronzed grackles from Ann Arbor were 138.0 and 142.5 mm., respectively. The ratio of the Michigan mean weight to the Florida mean weight is 1.11; the ratio of the cube of the Michigan mean wing length to the cube of the Florida mean wing length is 1.10.

The culmen length (chord of dorsal profile of bill) shows a rather different pattern of variation from that of the wing length. This is shown in Table II and Figure 2.

The culmen decreases in length northward, but the scatter of the means for each degree of latitude about the fitted regression line is much greater relative to the variance within groups than is the case for the wing length, being significant at the 0.001 probability level. This shows that this simple regression line greatly over-simplifies the picture. If the culmen length is strongly affected by selection based on the nature of the food eaten, one would not expect a simple linear relationship to the latitude. The fact that the

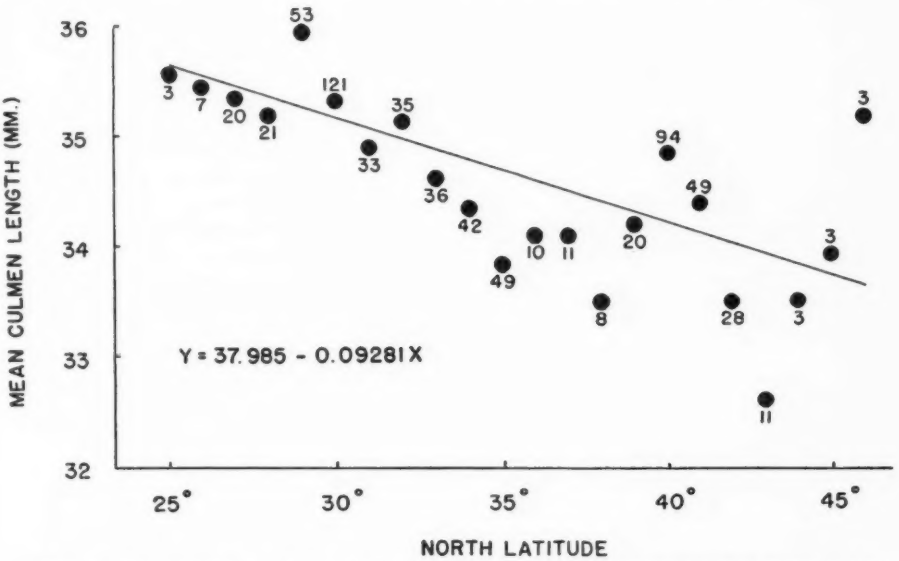


FIG. 2. Regression of culmen length on latitude. The numbers by the circles indicate the number of specimens in each degree group.

Louisiana birds have longer bills than the Florida ones might be an indication that the ratio of bill size to body size is fairly constant for grackles in southern coastal regions. The ratio of Louisiana mean culmen lengths to Florida ones is 1.020; the comparable ratio for wing length is 1.017, which suggests that this surmise may be correct.

It was felt that the geographical pattern of the deviations from the straight regression lines on latitude might be of considerable interest in showing whether there was any relationship between the linear measurements and the distribution of the color forms. The measured specimens were arbitrarily divided into geographical blocks in such a way as to utilize as many of the specimens as possible without using any blocks containing fewer than five specimens. The mean wing and culmen lengths and their standard errors were calculated for each of these blocks. The expected values of the measurements for the latitude of the center of the block, calculated from the regression formulae, were subtracted from these observed means. The ratios of the differences to the standard errors of the observed means were compared with a table of the distribution of t (Fisher and Yates, 1948) to find the probability that the deviation from expectation was random. These deviations are represented by circles in Figure 3 for the wing and in Figure 4 for the culmen. On the wing-length map, 10 of the 41 samples differ significantly ($p<0.05$) from expectation. Four of the five significant negative deviations are in samples composed entirely of the end forms, *quiscula* or *versicolor*, while four of the five significant positive deviations are in samples composed largely of intermediates. The map of the culmen deviations is similar. Thirteen of the deviations are statistically significant. Five of the seven significant negative deviations are in samples largely or entirely *versicolor*, and all six of the significant positive deviations are in the intermediate zone.

The geographical distribution of these deviations suggested that it would be desirable to test the partial regression of the measurements on the latitude, the longitude, the position on the *quiscula-versicolor* cline, and the condition of intermediacy in color as contrasted with the end forms. At the suggestion of Mr. M. H. Quenouille the latter two variables were represented by two dummy variates. The "hybrid variate," u, has a value of 1 for *versicolor* and *quiscula* and a value of 0 for *ridgwayi* and *stonei*. The "clinal variate," v, has a value of 1 for *versicolor*, 0 for *ridgwayi* and *stonei*, and –1 for *quiscula*. The latitudes, longitudes, hybrid variates, and clinal variates used in this computation were those for birds whose culmens were measured. The statistical procedure is outlined in the appendix. The resultant analysis of variance of the wing length is shown in Table III.

The interpretation of this is that wing length shows a highly significant regression on latitude, increasing northward, and a highly significant difference between the end forms and the intermediates, the latter being larger. Each of these tendencies is maintained when allowance is made for the existence of the

TABLE III—ANALYSIS OF VARIANCE OF WING LENGTH

VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	VARIANCE RATIO	PROBABILITY
Due to latitude	1	825.47	825.47	27.0	<0.001
Due to longitude	1	93.19	93.19	3.05	>0.05
Due to u	1	232.00	232.00	7.60	<0.01
Due to v	1	46.46	46.46	1.52	>0.2
Error	663	20,245.59	30.54		
Total	667	23,870.08			

other. But when allowance is made for the existence of these tendencies, there is no significant regression on longitude or on the cline from *quiscula* to *versicolor*.

The analysis of variance of the culmen length is shown in Table IV: one can see that the results closely parallel those for the wing length, except that the regression on latitude is negative, meaning a decrease northward. Again the intermediates are larger, on the average, than the end forms. The variance ratio for *v* actually indicates a strikingly small difference between the groups.

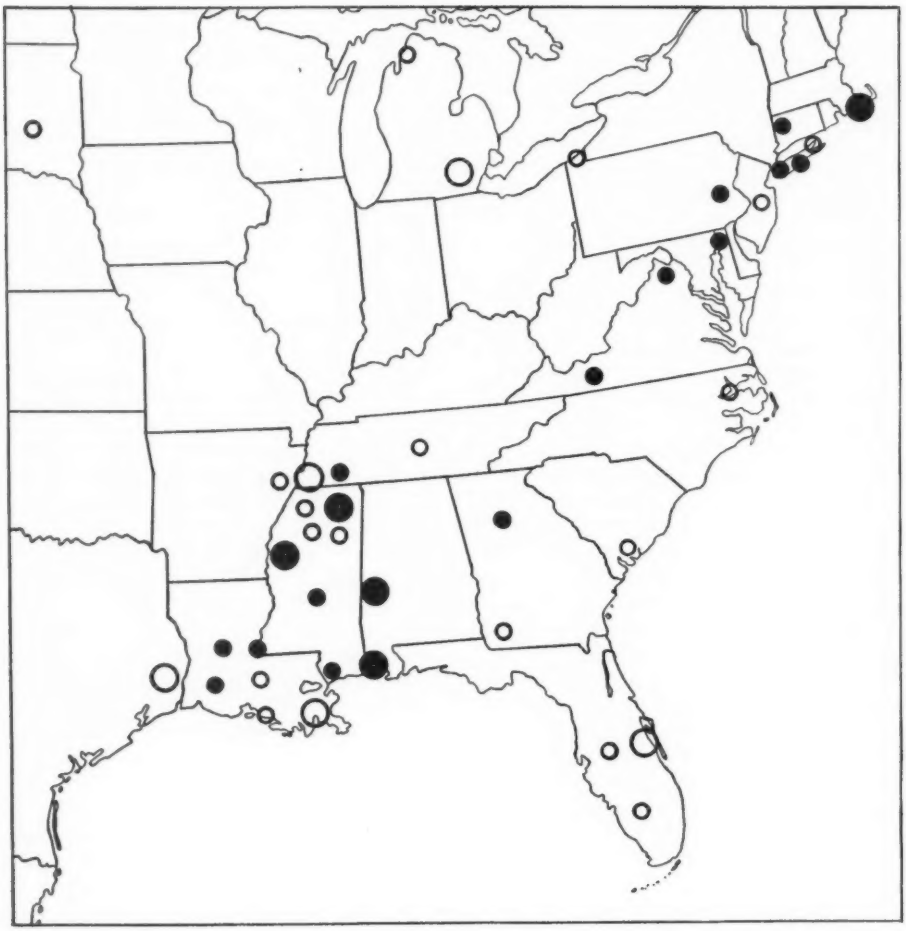
A comparison between groups within the intermediates showed no consistent significant differences between *stonei* and similarly colored birds on the one hand and *ridgwayi* and similarly colored birds on the other hand in Louisiana and in Mississippi. This is true with respect to wing, culmen, tail, tarsus, and wing-tip measurements (Huntington, 1952). The same holds true for the wing length, the only measurement so analyzed, in New Jersey and southeastern Pennsylvania. A discriminant function based on the wing and culmen and weighted to distinguish between *stonei* and *ridgwayi* did not show a difference between them significant at the 0.05 probability level.

The northward increase in the wing length, probably reflecting a trend in the general size, is apparently an example of Bergmann's principle that warm-blooded animals of a given body plan tend to be larger in the higher latitudes, thereby reducing the heat loss by decreasing the ratio of the surface area to the volume. The southward increase in the bill length, although it was used by Allen (1871) in formulating his rule of the northward diminution in the size of projecting parts, is probably not an adaptation of the volume-to-surface ratio to different heat economy requirements. Heat loss from a horny-sheathed bill would be negligible compared to that from the vascular skin, as in the ears of hares. In view of the well-known plasticity of birds' bills in adaptation to food habits, it seems probable that the cline in bill length is an adaptation to the southward increase in the proportion of soft foods in the birds' diet.

The larger size of both the wing and bill in the intermediates suggests the phenomenon of heterosis, which implies that selection favors larger size or qualities associated with it, either in the region occupied by the intermediates or throughout the range of the species. The parent populations were not inbred in the sense that domesticated strains are inbred, but they were inbred in the sense that they were isolated from each other, permitting each to acquire genes which the other lacked. The reunion of these parent populations resulted in a hybrid population with a higher degree of heterozygosity than either parent population. It is conceivable that the mere possession of certain heterozygous loci or of a relatively high proportion of heterozygous loci could produce a form of heterosis in which the larger size of the heterozygotes was due simply to their heterozygous nature, but the usual explanations of heterosis involve the agency of selection, and it seems highly improbable that an extensive inter-

TABLE IV—ANALYSIS OF VARIANCE OF CULMEN LENGTH

VARIATION	DEGREES OF FREEDOM	SUM OF SQUARES	MEAN SQUARE	VARIANCE RATIO	PROBABILITY
Due to latitude	1	94.74	94.74	51.6	<0.001
Due to longitude	1	5.91	5.91	3.22	>0.05
Due to *u*	1	44.96	44.96	24.5	<0.001
Due to *v*	1	0.0035	0.0035	0.0019	ca. 0.965
Error	658	1,207.42	1.83		
Total	662	1,432.12			



POSITIVE NEGATIVE

SIGNIFICANT AT 5% PROBABILITY LEVEL

NOT SIGNIFICANT AT 5% PROBABILITY LEVEL

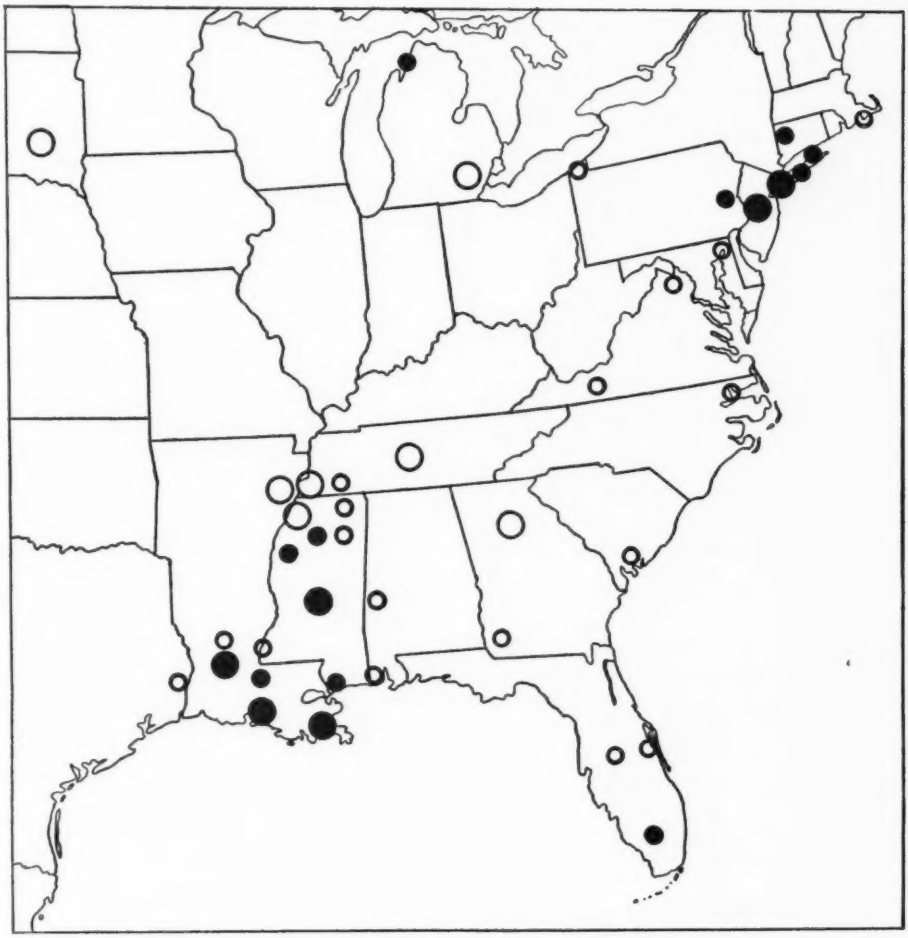


FIG. 4. Deviations from expected culmen length. Symbols as in Figure 3.

FIG. 3 *(opposite)*. Deviations from expected wing length.

mediate population averaging larger in size could arise without the aid of selection.

Absence of Reproductive Isolation

Due to technical difficulties the evidence from the field concerning the reproductive relationships between the differently-colored forms is rather scanty. This evidence is of three kinds: the composition of a mixed breeding colony as determined by trapping, the identification of mixed breeding pairs by means of colored bands, and the occurrence of differently colored siblings in a single brood.

In 1950 and 1951, at a mixed breeding colony in Hamden, Connecticut, 54 birds were either trapped in May or seen in May after being color-banded in April. These included 30 males and 24 females; this is the largest sample identified in a single mixed breeding colony of grackles which has come to the author's attention. At the time of trapping the sex and a code number for the back color were recorded. The code followed as closely as possible the system used by Chapman, in which *Q. q. quiscula* was designated by *1*, *stonei* by *2*, *ridgwayi* by *3*, and *versicolor* by *4*. It is almost impossible to assign these numbers to the duller females, although they always have heads bright enough to classify. A number was assigned to every female trapped, but it was often a very rough approximation. The sex was determined chiefly on the basis of the smaller size of the females, usually corroborated by duller plumage. The distribution of back colors in the colony is shown in Figure 5. Due to the subjective nature of the classification system and its application, the form of the distribution curve has little significance. What is significant is that in this single breeding colony the species varies without interruption from purple-backed individuals through brassy-green to bronze.

The difficulty of identifying band combinations in the field, aggravated by the

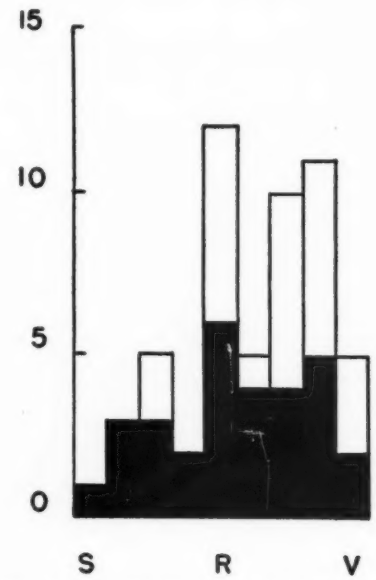


FIG. 5. Classification of grackles trapped in breeding colony at Hamden, Conn. Black = males; white = females. *S* = *Q. q. stonei*; *R* = *Q. q. ridgwayi*; *V* = *Q. q. versicolor*.

lack of territorial behavior, thwarted the plan to find out whether mating is random with respect to color, but on May 6, 1951, a male and female, both banded, were identified away from the colony, flying and feeding together as if mated. The male had been recorded as *2½* (*stonei-ridgwayi* intermediate); the female was dull and had been hard to classify, but had been recorded as *3½*. On April 15, 1951, a male, banded and recorded as *2¼*, was seen away from the flock, similarly consorting with an unbanded female, which, through binoculars at 50 feet in a very good light, appeared to have a bronze back with none of the iridescent bands which characterize the dorsal feathers of *ridgwayi* and *stonei*. These two differed strikingly in head color, too, the male having a greenish-blue head and the female a bluish-purple head.

The third piece of evidence on the question of interbreeding and lack of repro-

ductive isolating mechanisms is provided by two broods of grackles taken from the nest and kept in captivity until the molt into adult plumage was completed. In one brood of five two males were as far apart as *3* and *3¾* on Chapman's classification scale. One of their sisters extended the span down to *2½*. The other brood contained only three birds, but the span was even greater. The two males were classified as *2½* and *3½*, while the female was called *2*. Thus the males alone of a single brood showed differences equivalent to those between the "subspecies" *stonei* and *ridgwayi*.

None of the three pieces of evidence described above, the spread of colors in the colony, the two apparently mixed matings, and the dissimilar birds from the same brood, conclusively proves that mating is random with respect to color, but taken together they strongly indicate that this is the case, at least in southern New England. Certainly they establish beyond question that forms as unlike as *stonei* and *versicolor* do interbreed. No reproductive isolating mechanisms have been demonstrated in the southwestern part of the intergradation zone, but the circumstances in Louisiana are more favorable to the development of such mechanisms, because of the contrasting environments of southern pine woods and cypress-tupelo swamps and the greater proximity of the extreme forms of this species.

The most probable kind of initial isolating mechanism, in species of birds in which the parents and young stay together after hatching, is the process of imprinting, which makes the young tend to mate with birds like their parents. This will prevent the formation of hybrids when two different populations first come together, but once an intergradation zone is established, this process will maintain it, with sharper edges than one would otherwise expect. There is nothing inherent in the process which would tend either to widen or to narrow the zone. Indeed, it would act as a stabilizer, resisting any such tendencies. To judge from the nature of the morphological differences in the European crows (Meise, 1928), which are differences in pigmentation, and those of the grackles, which are differences in interference colors and would rarely be apparent to nestlings seeing their parents against the sky, this kind of isolation due to imprinting is more likely to be effective in the crows than in the grackles. But when the young grackles leave the nest and can see other grackles from above, they are with a flock which may contain a variety of colors. If imprinting occurs at this stage, it could easily lead to mixed matings. This would tend to facilitate interbreeding between birds coming from colonies of similar composition, only impeding gene flow between radically different colonies.

Routes of Migration and Invasion

In the bird-banding office of the Fish and Wildlife Service at Laurel, Maryland, there are 10,700 punch cards representing recoveries of grackles through June 1949. Up to that time 140,320 grackles had been banded. These records offer an abundance of information on the migrations of grackles, which, cautiously interpreted, can be quite helpful in studying the post-glacial history of the species. It can not be assumed that migration routes will inevitably recapitulate post-glacial expansion routes. But unless other facts indicate to the contrary, the present migration route of birds breeding in a given region is more likely than any other to be the route followed by their ancestors in their invasion of that region. This is particularly likely to be the case when the migration route is not the easiest or most direct one.

The recovery data on the grackles were broken down according to the states of banding and recovery. The resultant table is unwieldy, but reveals considerable information when studied. The first gen-

TABLE V—INTER-REGIONAL MOVEMENTS OF BANDED BIRDS (INTRA-REGIONAL FIGURES ARE INTERSTATE MOVEMENTS ONLY.)

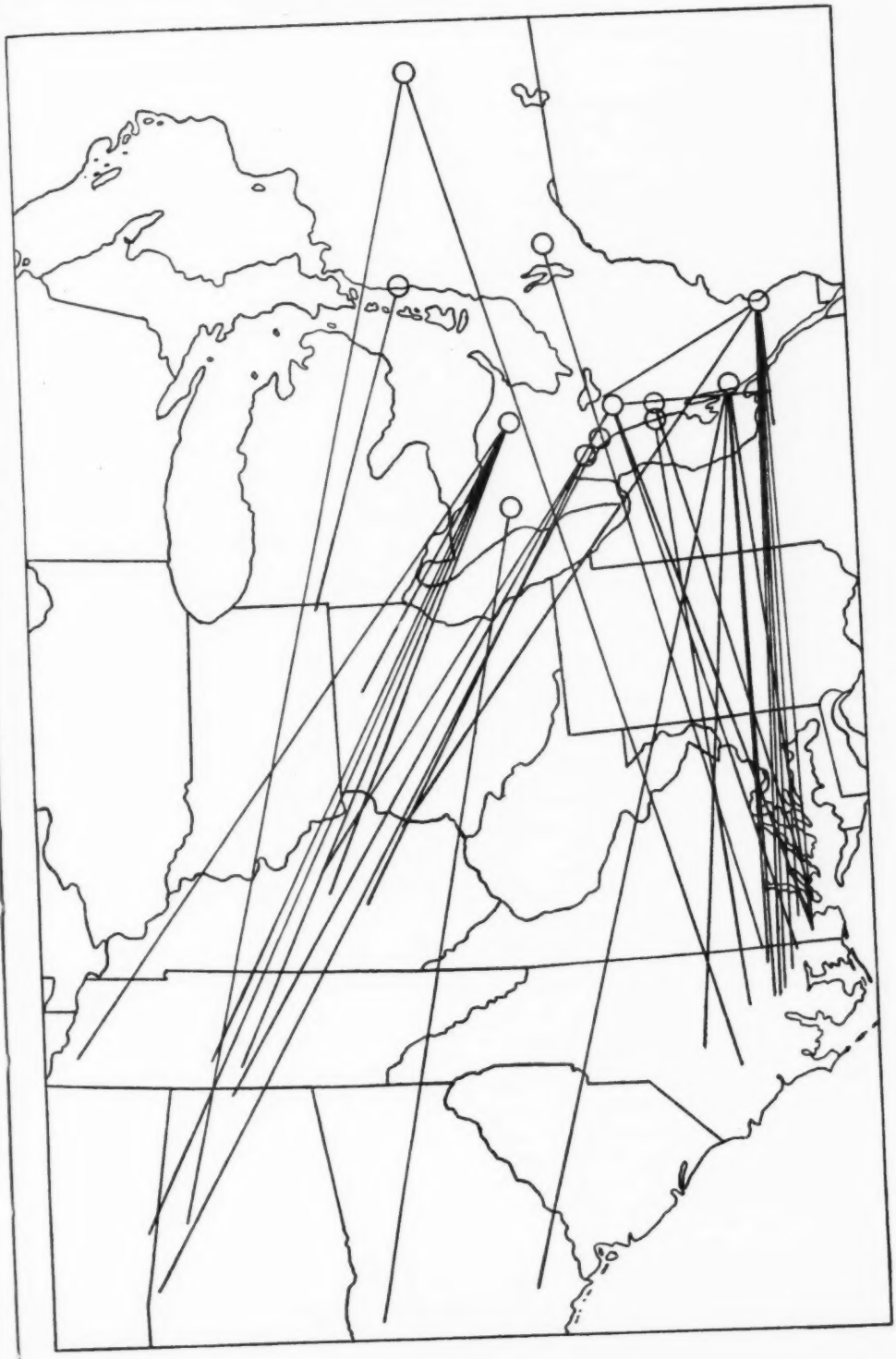
REGION OF BANDING	NE	SE	NC	SC	NW	SW	TOTAL
NE	154	100	9	5	0	0	268
SE	5	15	0	2	0	0	22
NC	8	26	196	593	52	157	1032
SC	1	6	63	36	33	21	160
NW	0	1	10	45	130	399	525
SW	1	0	25	32	724	338	1120
Total	169	148	303	713	939	855	3127

eralization which comes to mind is that there is very little crossing of the Appalachians. Table V, with the states grouped into six regions, shows this more simply, but less clearly. These regions are: Northeast—Maritime Provinces, Quebec, New England, New York, New Jersey, Pennsylvania, Maryland, Delaware, and District of Columbia; Southeast—Atlantic coast states from Virginia to Florida; North Central—Ontario, Michigan, Wisconsin, West Virginia, Ohio, Indiana, and Illinois; South Central—Kentucky, Tennessee, Alabama, and Mississippi; Northwest—Minnesota, Iowa, Missouri, Kansas, and the states and provinces north and west of them; Southwest—Arkansas, Louisiana, Oklahoma, and Texas.

Of 268 recoveries of birds banded in the northeast, 100, or 37%, took place in the southeast, while only 32, or 3%, of the 1192 recoveries of birds banded in the central regions occurred in the southeast. If the birds recovered in the same region in which they were banded are omitted from consideration, the percentages become 89% and 3%, respectively. Of the 24 birds which traveled between the northeast and the regions west of the Appalachians, at least 18 began or ended their journeys in Pennsylvania or New York localities west of the mountains. The only case of trans-Appalachian movement involving more than scattered individuals is that of the 17 birds banded in Ontario which were recovered in Virginia, North Carolina, and Georgia. The points of banding and recovery of birds banded in Ontario and recovered outside that province are shown in Figure 6. This map indicates that the breeding range of grackles which winter in southeastern Virginia and North Carolina extends not only across the northern Appalachians into Quebec, but also north of Lake Ontario and perhaps even north of Lake Huron. The two most northwesterly of these, banded at Kapuskasing and Tomiko River, Ontario, and recovered in North Carolina, were banded in September and so might have wandered northwestward after the breeding season.

If we bear in mind that much of that part of a bird's behavior which determines the choice of migration routes is probably learned, the lack of correspondence between the zone of intergradation and the line separating the breeding ranges of the populations wintering on the two sides of the Appalachians can be explained. After the breeding season is over, the grackles gather in flocks, foraging around the countryside. These flocks become larger as the migration begins. Birds whose ancestors migrated in the Mississippi flyway undoubtedly fall in with others which use the Atlantic flyway. If a population of birds which nest in a given region splits into two parts wintering in different regions, the more advantageous wintering grounds and their associated migration route will tend to sup-

FIG. 6. Recoveries of grackles banded in Ontario.



plant the others. Such a selective advantage might take the form of a higher survival percentage or of a route permitting arrival at the breeding grounds earlier or in better condition. This "capture" of breeding range will continue to advance until an equilibrium point is reached, where the selective advantage of one winter range over the other is negligible. In this way the northeastern populations of the bronzed grackle may have substituted the shorter migration route—across the northern Appalachians to the coastal plain wintering grounds—for the ancestral route to the Mississippi Basin wintering grounds.

The Status of stonei

Chapman did not consider the possibility that *stonei,* occurring in widely different environments from Louisiana to Connecticut, may be a continuation of the intergradation cline of *ridgwayi* instead of a distinct subspecies. He suggested (1936, p. 407) "that the changed population conditions incident to extension of range have permitted these mutational characters to find expression in *stonei,* which we may provisionally consider as neither the product of its environment nor of hybridism, but of heritable, individual variation, or mutation." However, it is the author's opinion that *stonei* is the product of both its environment, through selection, and hybridization. Reiterating his own belief on this point in 1940, Chapman said (p. 227) that the change from *quiscula* to *stonei* "is completed before the range of [*versicolor*] is reached. So far as existing conditions go, therefore, neither [*versicolor*] nor its descendants plays any part in the making of pure *stonei.*" This lack of overlap between *versicolor* and *quiscula* indicates that *versicolor* plays no direct part as a parent in the making of *stonei* at present, but it does not rule out the possibility of its playing a part through its descendants. When two genotypically different populations interbreed, the resultant population after several generations will form what Anderson (1949) calls a "recombination spindle," with the peak of the distribution halfway between the parent forms and tapering off on both sides; so that the parental recombinations are very rare, but there is a considerable proportion of the population which is very similar to each parent form. Without the operation of selection, there will always be individuals identical or very similar to one or the other parent form in any sample from an intergradation zone. However, selection can, and usually does, complicate the picture, and it is highly probable that it has done so here. Selection could produce populations lacking both end forms by causing certain genes to spread from one parent form into the other more rapidly than others, by ecological gradients, and by favoring the intermediates in the middle of the intergradation zone. No gradient or selective force has been found which would be effective across the intergradation zone everywhere from Louisiana to the Middle Atlantic States, but this does not mean that none exists, nor would it need to have the same basis throughout the length of the zone. On the other hand, there is apparently no physical barrier which could have split *quiscula* into two geographically isolated parts, permitting the northern one to develop into *stonei.* Even if southern Florida was once cut off by an arm of the sea, it must have been only at the height of an inter-glacial age, while the isolation required to split off *stonei* before it made contact with *versicolor* would need to have occurred as a glacial age began to wane.

Perhaps Chapman was influenced in regarding *stonei* as a distinct subspecies because it is the truly *purple* grackle and the commonest form along the Middle Atlantic coast and therefore has the weight of tradition behind it. He depicted its range (1936, p. 411) as constituting a much wider strip than that of *ridgwayi,* but the specimens examined in the present work indicate that *quiscula* breeds farther north and west in Georgia and

the Carolinas than Chapman believed, thus narrowing the range of *stonei* to a strip similar to that occupied by *ridgwayi*.

In studying museum specimens of this species, one is impressed by the range of variation included in the specimens classified by Chapman as *stonei*. He seems to have included more intermediates from both sides in *stonei* than in *ridgwayi*; even the type of *stonei* is definitely on the *quiscula* side of the distribution curve of specimens assigned to this form. This variability is, to the author, the most convincing reason for considering *stonei* to be, like *ridgwayi*, a part of the cline resulting from the interbreeding of *versicolor* and *quiscula*.

The intermediate forms pose a problem in nomenclature. The measurements and the color variation indicate that there is no sharp break anywhere in the morphological continuity of the species from *Q. q. quiscula* to *Q. q. versicolor*, which would serve to mark off intermediate subspecies. Since none of the measurements studied varies consistently with the colors by which these forms are distinguished, the colors themselves must remain the sole criterion. On this basis, no sample, large enough to be considered, from a locality within the intermediate zone is so homogeneous that 75% of its members can be distinguished as belonging to the "proper" subspecies for its locality. The closest approach to a "natural" classification would be attained by naming only the two end forms, *quiscula* and *versicolor*, as subspecies, but this would leave the populations of the intergradation zone nameless. As a practical matter, therefore, it seems most convenient to retain the names *"stonei"* and *"ridgwayi"* as intermediate types which serve as milestones along a cline, but without discrete ranges as geographical subspecies.

Adaptation to Climatic Differences

The post-glacial reunion theory is subject to the objection that it is by no means certain that the climate of the Gulf coast was at any time in the Pleistocene more rigorous than the present climate of the northern part of the breeding range of *versicolor*. But the possibility remains that the ancestors of *versicolor* were confined to warmer habitats than the present-day *versicolor* is. The grackles belong to a family of neotropical origin; therefore increasing adaptation to temperate habitats is in line with their recent evolutionary trend. The climatic differences to which *versicolor* and *quiscula* presumably became adapted during their isolation were differences in rainfall and seasonal extremes, the Texan and Mexican climate being drier and with more pronounced seasonal temperature contrasts, thus encouraging the development of a more euryecous nature and of the migratory habit. In Louisiana, *versicolor* now inhabits the drier country, which is in accord with this hypothesis about the climates in which its ancestors lived.

Width of the Intergradation Zone

Another difficulty encountered by the post-glacial hybridization theory is that the zone of intergradation is widest in the northeast, where there is the definite barrier of the Appalachians, and narrowest in Louisiana, where there is no barrier. The reasons for this phenomenon are undoubtedly complex, but some of them have become apparent in the course of this study. Dobzhansky (1941) explained this situation and that described by Meise (1928) in the crows *Corvus corone cornix* and *C. c. corone* by the assumption that the zone of intergradation will be narrowest where the duration of the contact has been longest, due to the formation of isolating mechanisms. In the case of the grackles, the width of the intergradation zone is so great that the existence of isolating mechanisms suppressing the formation of intermediates is scarcely credible.

Individual specimens of *versicolor* and *quiscula* have been collected within 40 miles of each other in Louisiana, and perhaps the forms may breed even closer. Yet the shortest distance between appar-

ently pure populations of these end forms, which is probably in the southern Appalachians, is probably not less than 200 miles and perhaps much more.

It must be remembered, however, that selection can promote the origin of isolating mechanisms in various ways. The most direct way would be the possession by the intermediates of lower fertility or viability than either parent form, due to genetic divergence of the parent forms during their separation. In the case of this direct selection against intermediates there would be a narrow zone from the beginning, without a zone occupied only by intermediates. The subsequent evolution of behavioral reproductive isolating mechanisms, further narrowing the zone, would be virtually inevitable. There is, however, no evidence of reduced viability or fertility of the intermediates in either the grackles or the crows. Even in the narrow intermediate zone of the crows, the "pure" end forms are almost entirely absent (Meise, p. 48), although there are many individuals which appear in the field to be end forms. The intermediate grackles thrive still more and are the sole occupants of large areas.

A second mode of origin of reproductive isolating mechanisms would occur if each of the "pure" end forms were better adapted than the other to the habitats found in its own range. Then, within each range selection would favor any mechanism which promoted inbreeding of these "pure" forms. If such a mechanism were a genetically determined preference in one of these forms for mates of that particular form, a narrowing of the intergradation zone would result. But the initial reproductive isolating mechanism is much more likely to be some such phenomenon as imprinting, in which certain adult behavior patterns are determined by stimuli received in a critical period early in the lives of the birds. This phenomenon is widespread and needs no mutation to make it function, causing birds of a feather to flock together. It tends to support the *status quo* and promotes matings between hybrids rather than backcrosses. Then, if the intermediates were better adapted than the end forms to any habitats, gene complexes embodying the phenotypic adaptedness of the intermediates would tend to become stabilized in those habitats. Immigration would tend to increase the variability of these intermediate populations, while selection would oppose this tendency. Under these circumstances the intergradation zone could widen until regions are reached where the intermediates are no longer better adapted than the end forms.

Dobzhansky's explanation that the intergradation zone is narrower where isolating mechanisms have had more of a chance to form is certainly not adequate in the case of the grackles. Mayr's emphasis on ecological factors (1942, p. 266) seems reasonable, in the absence of evidence of reduced viability or fertility in the intermediates. The narrowness of the zone in Louisiana can be accounted for in part at least by the contrast in habitats in that state, although it is not known in what way *versicolor* is better adapted to the drier oak-pine region and *quiscula* to the swampy delta region. In Mississippi and Alabama, where the contrast in vegetation does not accompany the contrast in the kind of grackles present, the intergradation zone is much wider. In the southern Appalachians the zone narrows again, presumably because of the mountain barrier. It then widens northeastward. The mountains in the north are lower and more penetrable than those of the south. The northern populations are more migratory than the southern ones, which would produce a greater degree of infiltration into each other's range in the northeast. The banding data have indicated that bronzed grackles from a large area in the northeastern United States and eastern Canada migrate through areas occupied by intermediates, and trapping by the author indicates that some migrants visit the sites of breeding colonies as they

do so. With the reduced degree of philopatry which one might reasonably expect to find among the individuals comprising the advancing front of a wave of territorial expansion, it seems not improbable that these circumstances could lead to more mixing in the north than in the south.

The possibility of better adaptation by the intermediates to the northern environment than to that of the south must also be considered as a basis for the difference in the width of the intergradation zone. In a changing environment like postglacial eastern North America the increased variability due to hybridization and introgression is undoubtedly an important factor in evolution (Anderson, 1949). The faster rate of change in the north, especially since the white man's arrival, has probably offered a more favorable environment for the survival of new recombinations in the "hybrid swarm" than the more slowly changing south.

Introgression

While both of the end forms show very little variation in their back colors throughout their ranges, the head color shows distinct polymorphism throughout the range of the species except for Florida. The green color may have originated in *versicolor* and spread through the intermediates, stopping short of the range of *quiscula*. But it seems unlikely that it would do this to the extent of establishing the same relative frequencies in *stonei* as in *versicolor* without becoming established in *quiscula* at all. A more plausible suggestion is that the introgression has been on the part of the purple color, originating in *quiscula* and either establishing a condition of balanced polymorphism in *versicolor* and the intermediates, advantageous to the species in a changing environment, or resulting in positive selection for intermediate blue heads over both green and purple. In any case, the head color has shown at least as much introgression as the back color and perhaps much more. It is quite possible that considerable introgression has taken place in less conspicuous characters.

What makes the grackles unique is that during their isolation in two refuges they acquired distinct morphological differences, but did not acquire chromosomal aberrations or other genetic differences which would diminish the viability or fertility of their hybrids, with the consequence that the recombinations are not bizarre, but provide a diversity which gives the hybrids an advantage over the parent forms in an unstable environment. The rarity of such intergradation zones without either parent form indicates that as a rule in wild populations effective reproductive isolating mechanisms arise before recognizable morphological differences do. The exception provided by the grackles is probably due to the fact that very minor quantitative differences in feather structure are observed because they result in disproportionately conspicuous differences in interference colors.

Summary

1. In an attempt to uncover evidence as to the evolutionary history of the purple grackle, *Quiscalus quiscula*, measurements of a series of 700 skins of males taken in May and June were analyzed statistically, records of 10,700 recoveries of banded birds were broken down geographically, 54 birds were trapped and color-banded in a mixed breeding colony, and the literature on the distribution, food, nesting site, and general habitat of the species was studied.

2. The euryecous nature of grackles has probably been a most important factor in their successful occupation of a large, varying range while they retain their identity as a single species.

3. The measurements of the wing and bill lengths show no correlation with the cline in back color from the bottle-green of the southeastern subspecies to the

bronze of the northwestern one. Both measurements show significant regression on the latitude, the bill decreasing and the wing increasing northward, and both measurements averaged larger in the intermediates than in the end forms. The bill-length variation is believed to be related to dietary differences and the wing length, indicative of size in general, to adaptation to heat economy. The larger size of the intermediates is apparently due to heterosis, favored by selection.

4. The color-banding in a mixed breeding colony gave evidence, through apparent mixed matings and the presence of differently colored siblings in hand-reared broods, but chiefly through the segregation in color within a single colony, that the differences in back colors do not act as reproductive isolating mechanisms.

5. The banding records showed that bronzed grackles nesting in the northeastern United States and eastern Canada do not follow the invasion routes of their ancestors in returning to their winter quarters: birds nesting as far west as central Ontario winter in the southern Atlantic coast states. This change is believed to be a learned habit favored by selection.

6. Chapman's theory of a post-glacial reunion between populations isolated in Florida and Texas is confirmed, except that *Q. q. stonei* is believed to be a result of secondary intergradation like *ridgwayi*, on the basis of its variability, the narrowness of the zone which it occupies, and the absence of a plausible explanation for its separation from the Floridian *quiscula* before the reunion with the western *versicolor*.

7. The width of the northern part of the intergradation zone is explained on the basis of several factors, especially increased mixing due to migration, selective forces favoring the intermediates in a rapidly changing environment.

8. The conspicuous introgression and the success of the hybrids are attributed to the occurrence of mutations producing striking color differences without the reproductive isolating mechanisms which usually accompany or precede such changes.

Appendix

The technique of partial regression (Fisher, 1946, pp. 156-166) is employed to test the significance of the effects of two or more independent variates on a dependent variate, isolating each of these effects by simultaneously eliminating their effects on each other. The amount of work increases exponentially with the number of independent variates. In the example to be given here the four independent variates are the latitude (x), the longitude (z), the "hybrid variate" (u), and the "clinal variate" (v). The culmen length (y) is the dependent variate.

A system of abbreviations for the sums of squares and products of the deviations from the means of these variates was set up as follows:

$a_{11} = [x^2]$	sum	of	squares	of	deviations	from	mean	of	$x =$	17,665.58
$a_{12} = [xz]$	"	"	products	"	"	"	means	of	$x, z =$	−14,275.87
$a_{13} = [xu]$	"	"	"	"	"	"	"	"	$x, v =$	−178.95
$a_{14} = [xv]$	"	"	"	"	"	"	"	"	$x, u =$	705.55
$a_{22} = [z^2]$	"	"	squares	"	"	"	mean	of	$z =$	40,219.90
$a_{23} = [zu]$	"	"	products	"	"	"	means	of	$z, u =$	625.91
$a_{24} = [zv]$	"	"	"	"	"	"	"	"	$z, v =$	955.82
$a_{33} = [u^2]$	"	"	squares	"	"	"	mean	of	$u =$	157.15
$a_{34} = [uv]$	"	"	products	"	"	"	means	of	$u, v =$	60.16
$a_{44} = [v^2]$	"	"	squares	"	"	"	mean	of	$v =$	241.51
$[y^2]$	"	"	"	"	"	"	"	"	$y =$	1,432.12
$[yx]$	"	"	products	"	"	"	means	of	$y, x =$	−1,639.51
$[yz]$	"	"	"	"	"	"	"	"	$y, z =$	514.72
$[yu]$	"	"	"	"	"	"	"	"	$y, u =$	−82.48
$[yv]$	"	"	"	"	"	"	"	"	$y, v =$	−133.44

The major step in a partial regression problem involving n independent variates is the solution of n sets of simultaneous equations, with n equations in each set. The sixteen equations in the present problem are set forth below:

$i=1$	$i=2$	$i=3$	$i=4$
$c_{11}c_{14}+a_{12}c_{24}+a_{13}c_{34}+a_{14}c_{44}=1,$	0,	0,	0
$a_{12}c_{14}+a_{22}c_{24}+a_{23}c_{34}+a_{24}c_{44}=0,$	1,	0,	0
$a_{13}c_{14}+a_{23}c_{24}+a_{33}c_{34}+a_{34}c_{44}=0,$	0,	0,	0
$a_{14}c_{14}+a_{24}c_{24}+a_{34}c_{34}+a_{44}c_{44}=0,$	0,	0,	1

The solution was obtained by the Doolittle-Fisher technique for solving reciprocal matrices, as described by Dwyer (1942).

The values of the c's thus found were:

$c_{11}=$	0.00013528	$c_{12}=$	0.00006156	$c_{13}=$	0.00016958	$c_{14}=-$	0.00068109
$c_{12}=$	0.00006156	$c_{22}=$	0.00005628	$c_{23}=$	0.00000007	$c_{24}=-$	0.00040261
$c_{13}=$	0.00016958	$c_{23}=$	0.00000007	$c_{33}=$	0.00745704	$c_{34}=-$	0.00235322
$c_{14}=-$	0.00068109	$c_{24}=-$	0.00040261	$c_{34}=-$	0.00235322	$c_{44}=$	0.00830997

For the solution of the matrix of c's the culmen measurements themselves do not enter into the calculation. Thus this same matrix of c's is valid in determining regression coefficients for any dependent variate, as long as the same group of specimens is involved.

The regression coefficients for the culmen length (y) were calculated by the formula:

$$b_i = c_{i1}[yx] + c_{i2}[yz] + c_{i3}[yu] + c_{i4}[yv]$$

The values obtained were:

$$b_1 = -0.11321 \qquad b_2 = -0.01824$$
$$b_3 = -0.57904 \qquad b_4 = -0.00537$$

These values were checked by the formula:

$$b_1a_{i1} + b_2a_{i2} + b_3a_{i3} + b_4a_{i4} = [yx_i],$$
where x_i is x, z, u, or v.

In the analysis of variance the sum of squares for the effect of each variate is b^2_i/c_{ii}. The reduced error sum of squares is $[y^2] - S(B^2)$, where $S(B^2) = S(b_i[yx_i])$. The analysis of variance is shown in the body of the paper as Table IV.

Since the effects of longitude and s were found to be insignificant, they should be eliminated in the final statement of the regression equation. This is done by replacing the c-matrix with one which includes only c_{11}, c_{12}, and c_{22}. One variate at a time is removed, using the formula:

$$c'_{ij} = c_{ij} - \frac{c_{ik}c_{jk}}{c_{kk}}$$ adapted from Fisher (1946), where k stands for the variate to be removed. In this case, however, since the desired matrix has only two rows and two columns, it is easier to use the specific formulae for constructing such a matrix afresh:

$$c_{11} = \frac{a_{jj}}{a_{ii}a_{jj} - a_{ij}^2}, \quad c_{ij} = \frac{-a_{ij}}{a_{ii}a_{jj} - a_{ij}^2}$$

The results are the same, regardless of which method is used. In this case,

$$c_{11} = 0.00005728 \qquad c_{12} = 0.00006521$$
$$c_{22} = 0.00643758$$

The regression coefficients are:

$$b_1 = -0.0993 \qquad b_2 = -0.6379$$

Using the coefficients thus obtained, the regression equation is:

$$Y = 34.7851 - 0.0993\,(x - \bar{x}) - 0.6379\,(u - \bar{u})$$

which reduces to:

$$Y = 38.4553 - 0.0993\,x - 0.6379\,u.$$

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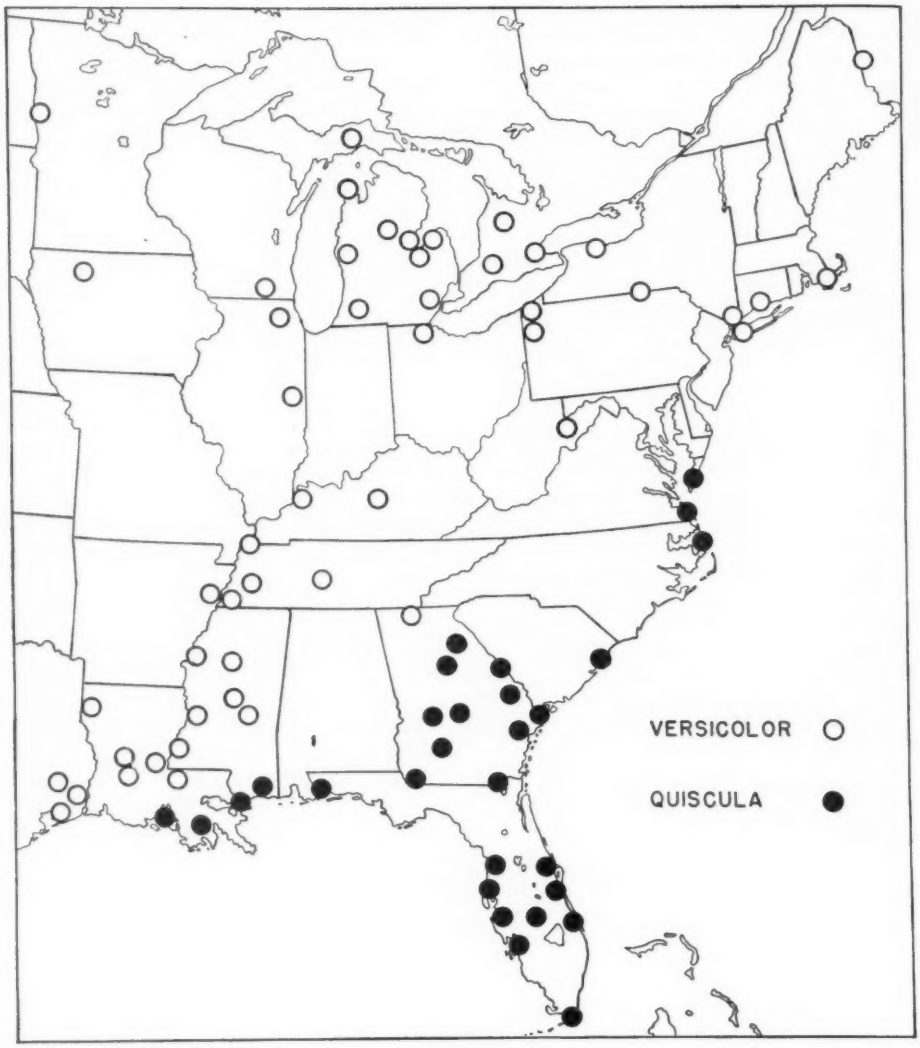
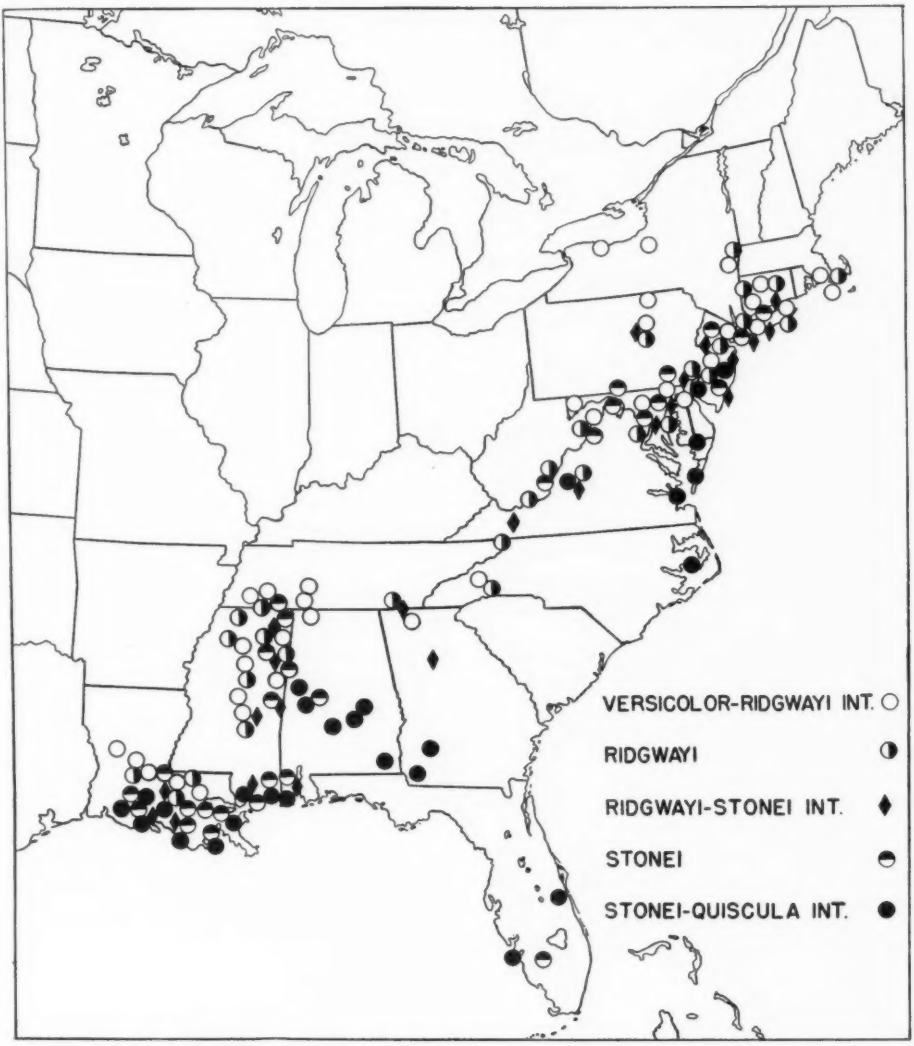


MAP 1. Distribution of *Quiscalus quiscula versicolor* and *Q. q. quiscula*. Symbols represent localities where specimens examined were collected.



MAP 2. Distribution of intermediates. Symbols represent localities where specimens examined were collected.

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CHARLES E. HUNTINGTON, of the Department of Biology, Bowdoin College, presented this dissertation to the Faculty of the Graduate School of Yale University in partial fulfillment of the requirements for the degree of Doctor of Philosophy. The author is indebted to many people for help of various kinds, but he wishes to express his gratitude particularly to Mr. G. E. Hutchinson, Dr. E. S. Deevey, Jr., and Dr. S. D. Ripley II for their advice and encouragement. The skins studied by the author came from the Academy of Natural Sciences of Philadelphia, the American Museum of Natural History, the Louisiana State University Museum of Zoology, the Museum of Comparative Zoology of Harvard University, the Peabody Museum of Natural History of Yale University, the United States National Museum, and the University of Michigan Museum of Zoology.

Symposium on Nomenclature

This symposium, the full title of which is "Problems of Nomenclatorial Practice now under Consideration by the International Commission," was held by the Pacific Section of the Society of Systematic Zoology at Corvallis, Oregon, on June 20, 1952. W. I. Follett presided. G. F. Ferris presided during the presentation of Mr. Follett's paper. The discussions which followed each of the papers can be found in the SSZ News section of the September issue of SYSTEMATIC ZOOLOGY.

Neotypes

ROBERT L. USINGER

THIS paper was prompted by the appearance of a preliminary consideration of the subject by the Secretary of the International Commission on Zoological Nomenclature (*Bull. Zool. Nomencl., 7,* 131-147, 1952). Secretary Hemming requests that the views of zoologists be sent to him by July 31, 1952 so that they may be considered while preparing the report which he intends to publish in the *Bulletin of Zoological Nomenclature* as a basis for discussion at the Copenhagen Congress (1953).

I have chosen to deal with this subject by first summarizing Hemming's views as expressed in the *Bulletin of Zoological Nomenclature,* and then offering a critique of his views. When considering the first part, readers are urged to refer to Hemming's original publication because my attempt to summarize his views may not in every case reflect his precise meaning.

Hemming's Discussion of Neotypes

1. Purpose: "The sole purpose of authorizing the establishment of neotypes is to provide a final and irrevocable standard for the identification of a species in cases where there is no surviving type material or where the known surviving type material is insufficient for this purpose." (*Bull. Zool. Nomencl. 8,* 140, 1952). Secretary Hemming adds that "The official recognition of a specimen as a neotype amounts in effect to the provision of an entirely new basis for the identification of the species concerned, while retaining for that species its original name with its original author and priority."
2. No specimen should be eligible for designation as a neotype unless it is, or becomes, the property of a museum.
3. Museums have a moral claim to the right to designate neotypes for lost types which were once in their possession.
4. Rigorous safeguards are needed in neotype designation to prevent exploitation by museums and by individuals.
5. The power to designate neotypes should be vested exclusively in the International Commission.
6. This central authority is needed to avoid competing or duplicate neotypes, to determine which institutions can be relied upon to take adequate care of neotypes, and to decide when to permit departure from original descriptions and from original type localities.
7. Neotype designation must be accompanied by a full description and figures.

8. Neotypes take precedence over any surviving type material.
9. Public notice should be given twelve months in advance of neotype designation.
10. The neotype should not run counter to any restrictions or restricted identifications and should agree with the appropriate subspecies in cases involving polypic species.
11. Neotypes should be substituted for the figures cited by the Commission (*Danaus plexippus* Linn., for example) as the basis for identification.
12. The International Commission should prescribe the information to be furnished in applications relating to the designating of neotypes.
13. Official neotypes should be differentiated by a code number such as "ICZN/1" from the unofficial neotypes established before the new scheme comes into operation, the unofficial neotypes to have no status in nomenclature.
14. The trivial name of every nominal species for which a neotype is established should be entered on the "Official List."

Critique of These Proposals

1. The neotype concept, originally confined to the replacement of lost types, has been expanded to cover cases where existing type material is insufficient to provide a standard for the identification of species. Worthy though this might at first appear, it introduces a new subjective element into the picture because there will be differences of opinion as to the adequacy of most type material. The action of the "first reviser" has sufficed in the past to take care of questions of this kind.

2-3. Museums are generally considered to be the rightful depositories for *all* primary types, it being generally agreed that such specimens, upon which scientific work has been based, are the property of science. Nevertheless, this is no more than a recommendation for holotypes, and it is not clear that neotypes are in any way more sacred than holotypes. It would appear that in the present state of international freedom in systematic zoology any central body that sets itself up to arbitrate the adequacy, competency and moral claim of competing museums and individuals is doomed to failure.

4-5. Exploitation of neotypes by individuals and institutions alike could presumably be prevented by action of the International Commission, just as exploitation of the secondary homonym procedure can be prevented. The difference between the present method of designating neotypes (by individuals) and the proposed method (exclusively by the International Commission) is that the individual taxonomist is provided with a procedure that he can use when he needs it, that is, when he is doing the work. By the other method a *formal* petition must be submitted to the already hopelessly overworked Commission. Under the present "unofficial" scheme the Commission would have to act only in the rare instances when actual abuses have been noted and have been submitted to it for ajudication.

This is the fundamental issue in all recent publications from the Commission. On the one hand statistics are provided to show that the Commission is swamped with more applications, and more letters each year than in all of the previous fifty years combined. On the other hand the trend, as in the present neotype recommendations, is away from so-called "automatic" nomenclature and toward central authority with the inevitable result that more and more cases must be referred to the Commission for arbitrary action.

6. Central authority is not needed to avoid competing and duplicate neo-

types, since priority of designation is a perfectly satisfactory criterion in such cases; no central authority should be charged with keeping the museums of the world under surveillance in order to insure that adequate care is being taken of neotypes. No central authority need decide when to permit departure from original descriptions or original type localities if the Rules were simply to require that neotypes be not inconsistent in characters and type locality with the data provided in the original description.

7. The requirement that a full description and figures be provided when designating neotypes is fine but is an "ideal" recommendation. Neotypes should be validated by the same requirements as holotypes, i.e. the provisions of Article 25, the Law of Priority.
8. If there is any surviving type material there is no excuse for neotypes.
9. Public notice could serve a useful function in neotype designation and still not require formal action of the Commission and twelve months' public notice in the *Bulletin of Zoological Nomenclature*. A method might be to allow a twelve months' period, or even longer, *after* the designation of a neotype by a specialist during which period the neotype is open to challenge. If at the end of the specified period no protest has been received by the Commission, the neotype is deemed to have been accepted by taxonomists and is not subject to change without formal action by the Commission.

10-11. These appear to be obvious principles which should be a part of any sound neotype concept.

12-13. Neither of these ritualistic proposals is relevant unless the International Commission is made the central authority for all neotype designations.

Secretary Hemming's Fourth and Fifth Problems

ELLSWORTH C. DOUGHERTY

THE problems here under consideration are respectively:

1. The species to be accepted as the type species of a nominal genus, the name of which was first published in a generic synonymy; and

2. The application of a trivial name which, when first published, is applied to a particular species or specimen, but which is stated (or implied) also to be a substitute name for some previously published name.

The foregoing two problems are relatively minor ones, but provide a useful opportunity for examining certain Articles in the *Règles* more critically. It is my feeling that both can be solved quite simply by extending to them the application of these Articles. In neither case has Secretary Hemming in his respective papers on these problems (*Bull. Zool. Nomencl., 7*, 109-120, 121-130, 1952) raised the specific points that I am presenting here.

In my own systematic work I have never found it necessary to decide a case in either category. What I have to say, therefore, is in the nature of a theoretical evaluation of the problems.

It seems logical that, wherever possible, problems not explicitly or clearly soluble under the existing *Règles* (even as extensively emended at Paris) should be examined with the aim of finding, in the already established body of nomenclatorial law, principles that can be extended to cover the problems in question. It seems likely to me that such will in most cases accord best with general practice, for it is reasonable to suppose that the majority of workers have been guided in this way. At the same time it must be recognized that, wherever ambiguities or inadequacies exist in the *Règles* (and there have been many, of which only a part have as yet been resolved), differences in interpretation and procedure undoubtedly have arisen. Therefore, it will probably be necessary, after any new concept is formally established, that the International Commission review a certain number of cases where the new ruling would create unjustified confusion.

The species to be accepted as the type species of a nominal genus, the name of which was first published in a generic synonymy

As Secretary Hemming has pointed out in his paper, this question can arise only in connection with generic names published before January 1, 1951. He has actually put two questions to zoologists: first, whether a nominal genus should have any availability at all from the date on which it was first published in the synonymy of a previously published name; and, second, whether, in the event that the answer to this first question is yes, one should "(a) treat, as type species of the genus concerned, the species (whatever it may be) which is the type species of the nominal genus, with the name of which the generic name under consideration was synonymized at the time when it was first published, or (b) . . . regard, as eligible for selection as the type species of such a genus all the species cited on the occasion when the generic name was first published in the synonymy of another generic name."

In connection with the first of the foregoing two questions I take the view that a generic name published in a generic synonymy is automatically provided with an "indication" in the sense of Article 25 and should therefore be available for nomenclatorial purposes.

As regards the second question I propose a solution which I have suggested in a paper now in press in the *Bulletin of Zoological Nomenclature*. This involves the treatment of generic names first published in a generic synonymy as falling under the scope of Article 35, rule (f). The latter rule states (in the currently used English text): "In case a generic name without originally designated type is proposed as a substitute for another generic name, with or without type, the type of either, when established, becomes *ipso facto* type of the other."

Hemming has discussed this rule in a recent paper (*Bull. Zool. Nomencl., 6,* 45-48, 1952). He has pointed out that it is ambiguous in certain respects and has proposed that it be redrafted to read: "In case a generic name without originally designated type is expressly published as a substitute for some specified generic name of older date with or without type, the type of either, when established, becomes *ipso facto* type of the other."

I have perceived still another defect in rule (f), which I have set out in the paper mentioned; the latter is submitted herewith as an Appendix. From paragraph 8 of this Appendix it may be seen that the publication of a generic name for the first time in a generic synonymy might be conveniently regarded as tantamount to the express publication of that name as a substitute for the earlier generic name, in the sense of Hemming's redrafted Article 30, rule (f); it would then follow that any limitation to be placed on the species eligible for selection as type of a substitute genus should apply to the special case of generic names originally published in generic synonymies. Thus,

in effect, I agree with the first of Hemming's alternative proposals, namely that one should "treat, as type species of the genus concerned, the species (whatever it may be) which is [or is selected to be] the type species of the nominal genus, with the name of which the generic name under consideration was synonymized when it was first published."

Since writing my paper on Article 30, rule (f), I have come to recognize a further difficulty with this rule upon which neither Hemming nor I have commented. It occurs to me that under certain circumstances it may be difficult to apply this rule even though ostensibly it would be invocable within the restrictions imposed by both Hemming's and my proposed emendations. There are certainly situations in which a given author may specifically propose a generic name as a substitute for some specified generic name of older date, (and this would, on my interpretation, include generic names first cited in synonymy of an earlier genus), yet where the later generic name would clearly apply to quite a different generic concept because, in an extreme case, of previous elimination of all the species included in the original nominal genus. Thus, there might arise a situation in which the later generic name would be quite inappropriate as a substitute for the earlier nominal genus—by reason, for example, of its etymology, which might connote attributes not possessed by the species or group of species, to which, perforce, it would have to apply.

In answer to this objection may be cited Article 32, which reads "A generic or a specific name, once published, cannot be rejected, even by its author, because of inappropriateness." With this concept already firmly established in the *Règles*, the foregoing argument loses much of its force.

In conclusion, therefore, it is my feeling that a simple and desirable solution to the problem of determining type species for nominal genera first published in generic synonymies would be to construe them as falling under the provisions of Article 30, rule (f), as clarified and restricted by Hemming and by me.

The Application of a trivial name which, when first published, is applied to a particular species or specimen, but which is stated (or implied) also to be a substitute for some previously published name

It is obvious that a problem arises in this connection only when there are more than one species involved—that is, where the specimens on which the description in question is based belong in whole or in part to a different species from that for which the name is proposed as (or implied to be) a substitute. This point is implicit in Hemming's discussion, but is not clearly stated by him.

He proposes three possible solutions: "(a) a ruling that a nominal species [so] established is to be treated as a composite species and therefore that the species to which the new trivial name is to adhere should be a matter for determination under Article 31 [the first reviser principle]; (b) a ruling that in such a case two nominal species, bearing identical specific names (i.e., identical combinations of generic name and specific trivial name) are to be deemed to have been established, one of these names to be held to apply to the species bearing the rejected specific name, the other to the species described under the new name, the relative priority to be accorded to these two homonyms to be determined in accordance with the procedure laid down for determining the relative priority to be accorded to any pair of names published in the same book and on the same date; (c) a ruling that the new specific name in question is to be treated as applying automatically to the species bearing the rejected name (i.e., that such a name should be treated strictly as a substitute name), the other use of the new name being treated as having been due to a misidentification on the part of the author concerned."

Hemming favors the third of these solutions. His reasons are good ones, and I find myself in essential agreement. I will not review them here.

I should like, however, to fortify Hemming's stand by pointing out that this third solution is most in harmony with a part of Article 31 (the second sentence, which, according to a Paris decision, is to be incorporated into an otherwise much emended Article). This sentence reads ". . . a specific [trivial] name which undoubtedly rests upon an error of identification cannot be retained for . . . [a] . . . misdetermined species even if the species in question are afterwards placed in different genera."

There is a close parallel here between the problem under discussion and the situation described in the quoted sentence. In both cases a misidentification of two (or more) species is involved, but in one case an author finds it necessary to propose a new name. By analogy with the sentence in Article 31 the new name would follow the earlier species—that is, could not "be retained for . . . misdetermined species."

Mr. Hemming has also raised the question as to how to handle the subsidiary situation in which a new trivial name is implicitly applied on the basis of objective criteria as a substitute for an earlier name (as by the use of an earlier author's name or the locality of his type material in forming the new name) and at the same time applied to new material belonging to a different species. He seeks the advice of specialists as to whether the name should apply only to the earlier material. In my opinion such names should be so restricted.

In conclusion, I repeat my advocacy of Secretary Hemming's third solution to the general problem under consideration: namely, that a new specific name which, when first published, is applied to a particular species or specimen, but which is stated (or clearly implied) to be a substitute for some previously published name, is to be treated as applying automatically to the species bearing the rejected name.

Appendix

Suggested emendation of Article 30, rule (f) (rule relating to the type species of two nominal genera, of which one name is proposed as a substitute for an earlier name) *

1. In a recent paper (*Bull. Zool. Nomencl., 2,* 253-76, 1951) sect 4(b) I pointed out that Article 30, rule (f), has certain difficulties of application that the International Commission on Zoological Nomenclature do not appear to have recognized up to now. However, I did not specify what these were. It is the purpose of the present communication to enunciate the difficulties in question and to recommend an emendation of rule (f) so that they may be overcome.

2. In preliminary communication with Mr. Hemming, Secretary to the International Commission, I received through his kindness the text of a note proposing a certain clarification of rule (f) (*Bull. Zool. Nomencl., 6,* 37-44, 1951). I am in entire agreement with his recommendation. It does not, however, touch on the point with which I am concerned, and the present note may therefore be regarded as supplemental to his.

3. Article 30, rule (f), reads: "In case a generic name [=nominal genus] without originally designated type is proposed as a substitute for another generic name [=nominal genus], with or without type, the type of either, when established, becomes *ipso facto* type of the other."

4. The crux of the problem is that, while Article 30, rule (e)α, states that the "species which were not included under the generic name at the time of its original publication" are "excluded from consideration in determining the types of genera," Article 30, rule (f), ostensibly provides a way in which this provision

* From a paper in press in the *Bull. Zool. Nomencl.*

can be violated. Hemming has recommended clarification of the meaning of "proposed" in rule (f); but, even if his recommendation is adopted, the following situation can arise. Let us suppose that an author Jones erected a genus *A*—— with species *A*—— *x*—— and *A*—— *y*—— without designation of type. Later, Smith proposed genus *B* as a "substitute" for *A*—— (in the strict sense that Hemming suggests) and included species *B*—— *z*—— and *B*—— *w*—— along with Jones's species. Now let us suppose either that Jones designated or indicated *B*—— *z*—— as type of B, or that he designated no type and that the first reviser, Brown, subsequently selected *B*—— *z*—— as type of *B*——. In either case, according to Article 30, rule (f), *B*—— *z*—— *ipso facto* would become type of *A*——, *despite the fact that this was not an originally included species in* A. It seems to me that this is a situation that the framers of the *Règles* did not anticipate and would not have intended should arise.

5. *Opinions* 9, 35, and 148 involve the application of Article 30, rule (f), but do not consider the problem posed in paragraph 4. Of these, only *Opinion* 9 explicitly refers to rule (f). This *Opinion* has been canceled on the grounds that it gave no effective decision (see *Bull. Zool. Nomencl., 4,* 335, 1950). *Opinion* 35 has been canceled, except for historical purposes, and its decision amended and incorporated into the *Règles* (see *Bull. Zool. Nomencl. 4,* 66, 155-156, 166, 1950); *Opinion* 148 has been similarly treated (see *Bull. Zool. Nomencl., 4,* 147-148, 163, 164, 166, 1950). There appears to be no essential value in analysing these opinions here.

6. I hereby recommend that, in order to bring rule (f) into conformity with rule (3)*a* and with the rest of Article 30 generally, rule (f) be recast making it clear that the only species which can be the type species of what we may call a "substitute" genus (in Hemming's sense) is (1) the nominal species that is already the valid type species (by designation, indication, or selection) of the genus renamed, or (2) the nominal species designated, indicated, or selected for the substitute nominal genus, where the earlier nominal genus does not have a type species at the time the type for the substitute genus is so fixed and provided (and provided only) that the species so designated, indicated, or selected is one of the nominal species originally cited by the author of the earlier genus as belonging to that genus. (In other words, in the example given in paragraph 4, Jones or Brown must confine their choice of type of *B* to *A*—— *x*—— and *A*—— *y*——, the only species originally included by Jones in *A*, the genus for which *B* was proposed by Smith as a "substitute" genus.) In foregoing case (2) the species in question *ipso facto* also becomes type of the earlier genus. Where any other species is designated, indicated, or selected for such a "substitute" genus, the choice so made is invalid.

7. The International Commission is hereby petitioned to recommend the foregoing change in Article 30, rule (f), to the next (XIV) International Congress of Zoology.

8. The attention of the International Commission is also drawn to the fact that rule (f) has implications for the problem of generic names first published in generic synonymies. The question as to how type species should be selected for nominal genera bearing such names is at present *sub judice* and the Secretary to the International Commission is to make a report on the subject to the next (XIV) International Congress of Zoology (see *Bull. Zool. Nomencl., 4,* 351-352, 1950. If it were to be decided that a generic name cited in a generic synonymy should be construed as a "substitute" for the earlier generic name in the sense of rule (f), then such generic names could come under Article 30, rule (f). It would seem to me that this would provide a reasonable solution to the problem now *sub judice*.

Emendation of Zoological Names

W. I. FOLLETT

IN AN article by Secretary Hemming entitled, "The Emendation of Zoological Names: an Appeal to Zoologists for Advice," (*Bull. Zool. Nomencl., 7,* 4-60, 1952) the problem of emendation is set forth with great particularity. It may be summarized as follows:

Article 19 of the International Rules of Zoological Nomenclature, enacted at the Berlin Congress of 1901, provided that "The original orthography of a name is to be preserved unless an error of transcription, a *lapsus calami,* or a typographical error is evident." (cf. *Proc. Biol. Soc. Wash., 39,* 80, 1926.) This provision has received extremely diverse interpretation.

It is proposed to substitute a provision based upon strictly objective criteria. Such a provision should recognize the recent decline of classical training among zoologists. The scheme should be a compromise, in order to please as many workers as possible. A scheme is suggested below, and its criticism by specialists is requested.

Definitions

Emendation: Any change in the spelling of a previously published scientific name, which the author of that spelling change makes clear is intentional. (Hemming, 1952, 21, par. 17)

Erroneous spelling change: Any change in the spelling of a previously published scientific name, other than an emendation. (21, par. 17)

A name that constitutes an "erroneous spelling change" shall possess no status in nomenclature; shall not be available as a replacement name; and shall not preoccupy any later use of the name. (23, par. 20)

This rule is subject to the proviso that the International Commission may declare valid a name that constitutes an "erroneous spelling change." (23-24, par. 21)

Valid emendation: An emendation made in accordance with any of the methods prescribed for the emending of names. (21, par. 18)

A name that constitutes a "valid emendation" shall replace the original spelling; shall rank for priority from the date on which the name was originally published in an incorrect form; and shall be attributed to the original author. (21-22, par. 19)

Invalid emendation: Any emendation, other than a valid emendation. (21, par. 18)

A name that constitutes an "invalid emendation" shall rank as a separately published name; shall rank for priority from the date on which it was published; shall be attributed to the author by whom it was published; shall be available as a replacement name if sufficiently distinct that it is not a homonym; and shall render invalid, as a junior homonym, any later use of that name. (21-22, par. 19)

Erroneous original spelling: The original spelling of a scientific name, when that spelling is later rejected in favor of a valid emendation. (21, par. 18)

A name that constitutes an "erroneous original spelling" shall possess no status in nomenclature. (21-23, par. 19)

Error of transliteration: Any spelling mistake due to the adoption of an erroneous method of transliterating a word into the Latin alphabet from some other alphabet. (47, par. 47)

Spelling mistake: Any erroneous spelling, other than an error of transliteration. (47, par. 47)

The Basic Rule

The original spelling of a scientific name is to be maintained, except as follows (47, par. 48):

1. In a name published after a designated date, a transliteration at variance with rules then in existence shall be corrected by subsequent authors. This exception does not apply to names of persons or places. (32, par. 29)

2. A spelling mistake may be corrected in a note or corrigendum published within twelve months, in the same volume, subject to review by the International Commission. This exception does not apply to errors of transliteration nor to names of persons or places. (cf. 36, lines 1-3) (34-36, pars. 32-33)

3. An error of transliteration or spelling mistake in the name of a person, deity, or place of ancient Greece or Rome shall be corrected by subsequent authors. (40-41, par. 39, Greek personage; 41-42, par. 41, Greek place; 41, par. 40, Roman personage; 42, par. 42, Roman place).

If there is disagreement as to whether a name falls within one of these categories, the problem of its emendation shall be determined by the International Commission. (42-43, par. 43)

These are the only exceptions. Any other kind of emendation may be made only by the International Commission. Specifically mentioned in this regard are emendations of the following:

1. Errors of transliteration, published before a designated date. However, it is recommended that such an emendation, in current use, should not be abandoned pending the Commission's decision. (28-29, par. 26)

2. Errors of transliteration in names published before a designated date but not emended before that date. (30-32, pars. 27-28)

3. A spelling mistake in a name published after a designated date. (38-39, par. 36)

4. An error in a modern place-name, irrespective of its date of publication, and despite any corrigendum published in the same volume. (43-44, par. 44)

Furthermore, where emendations of spelling mistakes not subject to automatic correction were published before a designated date, such emendations shall be subject to review by the International Commission. However, it is recommended that such an emendation in current use should not be abandoned pending the Commission's decision. (36-37, par. 35)

The temporary rule permitting automatic emendation of spelling mistakes based upon misreading of a label or register, should be reversed, and such emendation should be made only by the International Commission. This was the problem of *Leuciscus hakuensis*, based upon a misreading of the words "Lake Hakone" on a label, as discussed in *Bull. Zool. Nomencl., 4*, 143, 1950 (44-45, par. 45).

Supplemental Suggestions

In determining the proper spelling of a name as used for one taxonomic unit, the International Commission may, at the same time, determine the proper spelling of that name as used for other taxonomic units. (45-46, par. 46)

Emendations recorded in the Commission's Opinions and Official Lists shall be maintained. (48-49, par. 49)

Rules establishing the connective vowel to be employed in forming compound trivial names, and rules regarding the use of the letter "i" before the termination "-ensis," are to be considered separately from the subject of emendation. (50-51, pars. 53-54)

Discussion of the Suggested Scheme

The exceptions requiring the automatic emendation of names of personages and places in ancient Greece and Rome were avowedly made as a concession to zoologists trained in the classical languages. (41, par. 39). The advisability of maintaining these exceptions appears extremely doubtful. They are capable of involving serious uncertainties, such as (1) the

dates of the beginning and ending of Greek and Roman antiquity; (2) the boundaries of ancient Greece and Rome; and (3) variant spellings. This was implicitly recognized by the provision that in case of disagreement the problem of emendation should be determined by the International Commission.

Furthermore, it seems questionable whether the majority of zoologists who possess a classical training would be more offended, for example, at seeing the generic name *Lavinia* misspelled *"Lavina"* than at seeing the generic name *Adinia* misspelled *"Adina."* Yet one of these misspellings would be subject to emendation by subsequent authors while the other apparently would not. The determination of such matters might entail extensive and unprofitable expenditure of time.

In his recently distributed mimeographed paper entitled "The Emendation of Zoological Names," Sabrosky suggests three possible solutions to the problem of emendation. One of these he designates "The objective approach," although he expresses doubt as to the propriety of that designation.

I am convinced not only that this expression is exceedingly appropriate, but that the attainment of the highest possible degree of objectivity should outweigh all other considerations in attacking this problem.

For that reason, I doubt the sufficiency of the primary rule thus expressed in the proposal of Blackwelder, Knight, and Sabrosky (*Science, 108*, 37-38, 1948), "The original orthography of a name is to be preserved unless it can be demonstrated in the original publication itself that there has occurred an inadvertent error, such as a *lapsus* or a copyist's or printer's error."

As so expressed, that rule immediately gives rise to the question, "Demonstrated in whose opinion?"

For many years, ichthyologists were unanimously of the opinion that the trivial name *"equiselis"* was a typographical error for *"equisetis,"* yet a very old dictionary was eventually found to list both as variant spellings.

The American Ornithologists' Union's code for ornithology, which retains the original spelling, except for typographical errors, necessarily involves a subjective determination of what constitutes a typographical error.

With these uncertainties in mind, I have as yet found no means of expressing a thoroughly objective rule for the solution of this problem, except as follows:

The original orthography of a scientific name shall be preserved, unless the International Commission shall emend it.

Such a rule would require a definition of "the original orthography" that would provide for variant spellings in an original description.

This subject is considered in Hemming's paper, where the suggestion is tendered that if a name as first published was spelled in more than one manner, the spelling that is selected by the first subsequent author who uses the name shall prevail, provided (a) that the subsequent author is aware that more than one spelling was used, and (b) that from these he selects one to be the spelling employed. (49-50, par. 52). This is a modified version of the rule of the first reviser which was in effect for more than forty years.

I realize that this presents an extremely controversial issue, and that a decision either way will be disapproved by many colleagues whom I hold in high esteem.

However, I regard Hemming's suggestion as objectionable upon the following grounds:

1. The rule of page precedence has already been substituted for the rule of the first reviser, by enactment of the 1948 Paris Congress. (*Bull. Zool. Nomencl., 4*, 330, 1950).

2. In the Nomenclature Committee's recent questionnaire, a majority (57%) of the members of the Society of Systematic Zoology who voted, chose to accept page priority from January 1, 1954, onward.

This percentage includes those who chose to accept page priority as always binding.

3. In the same questionnaire, 35% of the members who voted indicated that they had refused to adhere to the rule of the first reviser even when it was in effect.

4. The suggested rule prevents a determination of the original orthography from the original description.

5. The suggested rule requires a search of literature that may not be readily available.

6. The suggested rule inevitably involves a doubt as to whether the *first* subsequent author has in fact been found.

7. The suggested rule necessitates the subjective determination of what constitutes a "selection" by a subsequent author.

The rule of page, line, and word precedence would therefore seem preferable in determining which of two or more variant spellings is to be regarded as the original orthography.

Perhaps an express corrigendum, published as a portion of the same volume, should outrank position-precedence in establishing the original orthography, although it may be doubted that the question of what constitutes an express corrigendum can always be determined objectively.

In closing, I wish to emphasize Sabrosky's recommendation "that the final proposal on emendations treat all names on the same basis, regardless of date." I concur heartily with his statement that "varying treatments . . . for names or emendations before and after the effective date of the plan [would] add a great deal to its complexity . . ."

W. I. FOLLETT, who presided at this symposium and was also a participant, is Curator of Fishes at the California Academy of Sciences.

ROBERT L. USINGER, of the Department of Entomology at the University of California, Berkeley, prepared his contribution to this symposium for the consideration of the American Committee on Entomological Nomenclature.

ELLSWORTH C. DOUGHERTY, of the Department of Zoology, University of California, Berkeley, wishes to express his thanks to Dr. Joel W. Hedgepth who read his paper at the symposium.

People and Projects

The President of Chile has conferred upon Charles P. Alexander, of the University of Massachusetts, Amherst, the degree of Commander in the order "Al Merito Bernardo O'Higgins" in recognition of his comprehensive accomplishments in the taxonomic study of the crane flies of Chile. The Ambassador from Chile, His Excellency, Felix Nieto del Rio, presented the medal, with a diploma and appropriate citation, by proxy at the Chilean Embassy in Washington on October 17, 1952 before a group of Dr. Alexander's friends, the latter being unable to attend due to a recent extended collecting trip in Alaska.

J. Linsley Gressitt, Lingnan University and the University of California, has been engaged by the Pacific Science Board of the National Research Council to coordinate the preparation, identification, and distribution of the Insect Survey Collections of Micronesia. These collections, which form a fairly comprehensive survey of insects in the Micronesian area, were made by participants in the ICCP (Invertebrate Consultants Committee for the Pacific) program of the Pacific Science Board. This program is carried on with financial assistance from the Office of Naval Research. Dr. Gressitt has visited the various museums in this country where these insect collections are now in various stages of preparation and has worked out plans for distribution of the material to specialists for identification and study. A tour of the museums was made last spring, and possibly a second one will be required later. His headquarters are at the Bernice P. Bishop Museum, Honolulu. It is hoped that the reports will be published in a series entitled "The Insects of Micronesia" by such an institution as the Bishop Museum.

Dr. Gressitt would like to borrow for study any Micronesian insect material in museums or private collections and to know of specialists willing to study part of the material.

P. J. Clarke has extended the coefficient of divergence, previously suggested by Klauber as an objective estimate of the divergence between two populations, for use with multiple characters. This paper (*Copeia*, 1952, 61-64) suggests a slight modification of Klauber's formula and gives a generalization of the formula for the simultaneous treatment of any number of characters. Systematists will find this statistical technique of wide utility.

The Committee on Nomenclature of the Society of Systematic Zoology is now preparing for the Fourteenth International Congress of Zoology, to be held at Copenhagen in August 1953. This will be a critical meeting for taxonomists, because Secretary Hemming's new code will probably be presented there for final action. The Congress is to be preceded by a Colloquium on Nomenclature, to which certain zoologists will be invited by the Commission's secretary. The committee is taking steps to assure that American taxonomists will be adequately represented, by qualified nomenclaturists. It wishes to be informed of the names of any persons planning to attend the nomenclature sessions. (Write to W. I. Follett, California Academy of Sciences, San Francisco 18, Calif., who is chairman of this committee.) Cooperation with the committee can be helpful to an individual, since only official delegates will be admitted to some sessions, and there are a limited number of organizations entitled to name official delegates.

RESEARCH MATERIAL WANTED

Bembicinae and Stizini (Hymenoptera: Sphecidae) from the New World, for revisionary treatment. Specimens returnable upon request or at end of project. James E. Gillaspy, Zoology Dept., University of Texas, Austin 12, Texas.

SSZ News

ABSTRACTS

The following abstracts of the papers to be presented before the Society of Systematic Zoology at its December, 1952 meeting in St. Louis are arranged in order of presentation.

1. SPECIATION AMONG THE OPILIONES (ARACHNIDA). Clarence J. Goodnight, Purdue University.

The suborder Laniatores of the arachnid order Opiliones contains a number of extremely variable species. As a result of this variability, a large number of invalid genera and species have been established for the rich neotropical fauna. Structures which show variability within a single species include dorsal armature, leg development, and even the number of tarsal segments. While a few isolated species show little of this variability, most are widespread and exist as polymorphic forms. In a few cases, it has been possible to demonstrate populations breaking up into subspecies. Thus variation, geographical spread, and isolation all play their role in the formation of opilionid species. The recognition of these facts results in an entirely new concept of their classification. Fewer genera and species are recognized, but many of these are divisible into distinct populations and subspecies.

2. CONVERGENT EVOLUTION AMONG THE NYMPHS OF MAYFLIES (INSECTA: EPHEMEROPTERA). George F. Edmunds, Jr., University of Utah.

The nymphs of mayflies seem to illustrate the phenomenon of convergent evolution to a remarkable degree. Numerous examples of parallel adaptation are known to occur in the order, especially among those nymphs living in torrents, in heavily silted streams, or on sandy bottoms. Among the mayflies convergent evolution occurs commonly among inhabitants of ecologically rigorous habitats while forms with greater structural diversity which have undergone little parallel adaptation occur together in more favorable habitats.

3. THE POSSIBLE OCCURRENCE OF RANDOM FIXATION IN A POPULATION OF FLEAS. Harold E. Stark, U.S. Public Health Service.

Thrassis bacchi is a species of flea occurring in nearly all areas of western United States parasitizing ground squirrels of the *Citellus leucurus* group. At the present time there are seven recognized subspecies. There is conclusive evidence that clines exist between all except one which is ecologically isolated by its preference for the vole, *Lagurus curtatus*.

In San Juan County, New Mexico, there occurs a group of this species limited to a very small area which has no visible external barriers. This group differs in morphological characters from neighboring populations of fleas and in no way follows the pattern of distribution of the species, or characteristics of a cline. Therefore, this phenomenon may be explained most logically by random fixation.

4. THE SUBSPECIES OF THE PURPLE GRACKLE, QUISCALUS QUISCULA. Charles E. Huntington, Yale University.

To shed further light on phylogenetic relationships within this species, measurements of museum specimens were analyzed statistically, recovery records of banded birds were studied, and birds in a mixed breeding colony were color-banded for individual identification.

Wing length increases northward; bill length decreases. Neither shows any significant relationship to the cline in color, but when the effect of latitude is removed, both measurements are larger in the intermediates than in the end forms.

The birds do not separate according to color in their choice of migration routes.

All evidence from color-banding and the hand-rearing of young indicates absence of reproductive isolating mechanisms.

No consistent ecological difference between the forms was found.

Chapman's theory of post-glacial hybridization between populations from refuges in Florida and Mexico is confirmed, except that *Q. q. stonei* is considered to be a result of this hybridization, not a geographic subspecies. The situation is unique because conspicuous characters have undergone introgression without the occurrence of reproductive isolating mechanisms.

5. GENERIC AND SUPERGENERIC CONCEPTS IN THE DIPTEROUS FAMILY SARCOPHAGIDAE. Harold R. Dodge, U.S. Public Health Service.

"Sarcophagidae" as a family name has existed at least since Bigot's time, based on

Sarcophaga Meigen. Today, some include sarcophagids with the Calliphoridae or "Metopiidae," and some still consider both to belong to the Tachinidae, but the sarcophagids are commonly considered a separate family. Further complications are the recognition of equivalent groups, such as Melanophoridae (Rhinophorinae), Rhiniidae and Stackelbergomyiidae.

Keys for separation of families and superspecific units within the Sarcophagoidea are satisfactory only when they deal with a limited fauna. Townsend's Manual is unique in that it deals with the world fauna, but his keys are difficult, and his arrangement is subject to controversy.

Sarcophagid generic and subgeneric names have increased tremendously since 1913. An incomplete survey reveals that there now have been approximately 587 generic and subgeneric names proposed in this family.

In the writer's opinion, thorough knowledge of the world fauna, including characters of males, females and larvae, is necessary to clarify concepts of genera and higher groupings. In the meantime, the generic name *Sarcophaga* will continue as a useful niche for a large part of the world fauna.

A list of generic names is presented. Generic, supergeneric and family concepts are discussed, and the classifications of several authors are reviewed.

6. What is a Species? M. W. De Laubenfels, Oregon State College.

(No abstract received.)

7. Serological Relationships among Some Distantly Related Arthropods and Mollusks. Charles A. Leone and Harry E. Webb, University of Kansas.

These serological studies confirm Lankester's contention that the king crab *Limulus* is more closely related to the scorpions than to the spiders. The decapod Crustacea showed some serological correspondence to the Arachnoidea. The Cirripedia and Isopoda, on the other hand, did not. Similarly, reciprocal testing yielded no measurable relationship between the Insecta and the other arthropods, or between members of the Annelida or Mollusca and the Arthropoda. The strong serological reactions between the reagents involving the two isopods *Armadillium* and *Bathynomus* may indicate a closer relationship among the various categories of isopods than among similar categories in groups such as the arachnids or decapod Crustacea. Among the Mollusca compared, the Cephalopoda appeared to be slightly more closely related to the Gastropoda than did the Pelecypoda. Both the "ring" test technique and a photoelectric turbidimeter were used to analyse these serological reactions. The former method is more sensitive than the latter method for detecting serological correspondence among distantly related organisms. The turbidimeter is more useful for differentiating among close relatives.

8. Serological Relationships of Some of the Families of Snakes. Elias Cohen, Rutgers University.

(No abstract received.)

9. Geographic Variability in the Common Hog-nosed Snake, Heterodon platyrhinos Latreille. Richard A. Edgren, Searle and Co.

Heterodon platyrhinos Latreille is distributed throughout the Eastern Deciduous Forest Formation, and westward slightly into the grassland. Ecological data suggest that the range is not continuous, but broken up into small partially isolated populations correlated with sandy soils. Four morphological characteristics vary geographically. The number of ventral plates and dorsal blotches vary clinally. Ventral counts are highest in the west-central portion of the species' range (Kansas, Missouri, etc.); these counts decrease gradually to the periphery with low extreme values in Massachusetts and southern Florida. Dorsal blotches are highest in the north and decrease toward the south, with lowest counts in southern Florida.

Material from Dade County, Florida is characterized by a high proportion of specimens lacking or with reduced azygous plates; through the remainder of the range less than 1% lack the plate. This population was recognized as a distinct subspecies (*Heterodon p. browni* Stejneger); it hardly seems worthy of continued recognition. About 9% of specimens are melanistic; black individuals are not generally distributed, but are most common in local areas, certain populations showing 75% or more melanism. Melanism is characteristic of older specimens. The trait is probably selected against.

10. Evolutionary Trends in Southeastern Asiatic Cyprinid Fishes. Martin R. Brittan, San Diego State College.

(No abstract received.)

11. Host-parasite Relationships in Avian Nasal Mites. R. W. Strandtmann, Texas Technical College.

As in many other groups of parasites, host specificity is very pronounced in the nasal mites of birds. There seems to be a correla-

tion between gregariousness of the hosts and the degree of similarity of the mite species.

Thus the anseriform birds, ducks, geese and swans, apparently all harbor the same species of mite, but in the strigiform birds, or owls, each species has a distinct species of mite. However, they may all be referred to the same genus.

Gulls and terns all harbor the same species of mite; so do the small wading birds that frequent seashores. Related wading birds that habitually remain aloof either have no nasal mites (killdeer) or they are of a different species (stilt).

The inference is that birds have had nasal mites since very early in their phylogeny and that new species arose only upon sufficient isolation of the host.

12. The Taxonomic Value of Male Genitalia in the Ithomiidae (Lepidoptera). Richard M. Fox, Colorado College.

With male genitalia of about 95% of the species at hand, their worth as taxonomic criteria in the Ithomiidae can be evaluated. They help establish interfamilial relationships but are not diagnostic of the family. Four of the eight tribes can, the other four cannot, be separated by genitalia alone. Tribal and familial definition must take into account all structures of the insects. Forty-three of the forty-five genera can be recognized through genitalia alone; the other two have identical genitalic structures which distinguish them from all other genera. The fifteen monotypic genera all are genitalically distinct. In twenty-three of the thirty polytypic genera all species are genitalically separable and in one of these genera even subspecies show recognizable differences. In five polytypic genera the genitalia separate species groups but not species, while in the remaining two genera genitalia are of no value for species determination. Mimicry within the family, parallel pattern evolution and striking color divergency in polytypic species all make difficult the recognition of true species and phylogenetic lines. Such problems have been resolved by analysis of structure other than color and pattern of wings, most frequently by use of male genitalia.

13. X-ray Photography as a Tool in Systematics. Robert Rush Miller, University of Michigan.

The value of Roentgen photography to biology was first demonstrated in 1896, the year after Roentgen discovered X-rays. Only since the 1930's, however, has the method been taken up by systematists. Its usefulness has been proven for the fields of paleontology, herpetology, and ichthyology; its scope is increasing in those fields; and it has potential application in entomology, malacology and other disciplines. With the startling technical advances of the past decade, it promises to become a more widespread and significant systematic tool in the near future.

Methods and techniques are explained and some results are illustrated. Relative costs are pointed out. The systematic value of the tool is reviewed for several individual fields of specialization and for some general studies, such as life history work.

14. Some Problems of Taxonomy within the Family Trypanosomidae (Protozoa: Mastigophora). Harley P. Brown, University of Oklahoma.

Generic determination within the family Trypanosomidae is based primarily upon two features: (1) hosts employed; (2) relative position of the basal granule and accompanying structures during various phases of the life cycle. If one finds in a culture of *Leptomonas* that some of the individuals have basal granules located slightly posterior to the usual anterior position, he is justified in transferring the classification of the organisms to the genus *Crithidia*. Or *are* we justified in using such flexible characteristics as the major or sole basis of generic distinction? Another weakness in the present scheme of classification is illustrated by intestinal flagellates of Southwestern lizards which are probably *Leptomonas* acquired from insects eaten by the lizards. Strict adherence to generic definitions would place these protozoa in the genus *Leishmania*, which seems absurd. Extensive life-history studies combined with experimental infections should aid materially in settling the problem, and will probably demonstrate the necessity for a re-definition of genera within this family.

15. The Lemnisci in Acanthocephalan Taxonomy. Harley J. Van Cleave and Jean A. Ross, University of Illinois.

History of the development of knowledge concerning morphology, ontogeny and physiology of the lemnisci in Acanthocephala will be traced. Features in these structures as distinctive for each order of the phylum will be presented. Conditions in the family Neoechinorhynchidae will be discussed with particular reference to uninucleate and binucleate lemnisci. Diversification in size of the lemnisci in the same individual has been studied for 14 species of the genus *Neoechinorhynchus*. In some instances conditions of these organs have distinct value in differentiating species.

16. An Analysis of the Shells of Freshwater Mussels Killed by Illinoian Indians of the Hopewell Culture. Max R. Matteson, University of Illinois.

Fresh-water mussels formed one of the chief items of diet of those North American Indians who lived along the waterways of the Mississippi River system during the Hopewell culture which existed roughly 500 years B.C. While excavating an Indian village on McGee's Creek near Meredosia, Illinois, Professor John McGregor, Department of Anthropology, University of Illinois, discovered large numbers of mussel shells in refuse dumps. The mussels had been used as food by these Indians. Among the shells were found farming implements and trinkets which had been made.

From these shells obtained, 15 genera including 24 species were determined. It was noticed that no thin-shelled mussels were present with the exception of one valve of *Strophitus rugosus*.

Several species seemingly occupied the Illinois River at that time but are not recorded as being present today. A present day survey of McGee's Creek adjacent to the village reveals a greatly changed molluscan fauna which suggests that significant changes have taken place in the physical aspects of the river during the past 2,500 years.

17. Names of Microtine Rodents. E. R. Hall, University of Kansas.

(No abstract received.)

18. The Making of Keys. Z. P. Metcalf, North Carolina State College.

A brief discussion of the author's present methods of making keys which have been developed during the past forty years. These methods, it is believed, are better than any previously developed. The primary purpose of keys is discussed and especial emphasis is placed upon clearness, distinctiveness of characters used, and mechanical means for facilitating the use of keys.

19. A New Cyprid Ostracod from Maryland. Edward Ferguson, Jr., South Carolina State Agricultural and Mechanical College.

A new species of fresh-water ostracod of the Acuminata group is described. It differs from related species in the structure of the genital lobes and of the ovary. Type locality: Eldon Hall farm near Princess Anne (Somerset County), Maryland. Type specimens: Two stained permanent mounts from which the description of the new species was made are deposited in the U. S. National Museum.

20. A New Subspecies of Bufo terrestris from Oklahoma. Arthur N. Bragg, University of Oklahoma.

The dwarf form of the American toad found in eastern Oklahoma and southeastern Kansas is given a name. Type locality: Cleveland County, Oklahoma. Holotype and paratypes in author's collection.

21. Some Observations on the Nature of the Genus. Ross H. Arnett, Jr., U. S. Department of Agriculture.

This paper is an inquiry into the philosophical nature of the genus, considering how the concept arose, discussing some of the questions which have arisen regarding the terms used in defining this category, and pointing out some of its characteristics. It is shown that there is existence of more than just individual things, by distinguishing between the taxonomic group (a particular genus), and the taxonomic category (the genus).

22. The Status of Various Kinds of Revisions of Type Localities. Hobart M. Smith, University of Illinois.

1. Revision of original statements of type localities is a necessity in taxonomy.

2. For the sake of clarity citations (other than original) should always be accompanied by indication of the status of that locality; (a) original designation, (b) subsequent designation, (c) subsequent selection, (d) subsequent restriction, or (e) subsequent correction.

3. The several sorts of alterations of type locality are better known as "revisions," not "corrections."

4. Subjective arbitration is needed on (a) the relative merits ("priority") of the various kinds of type locality revision; and (b) degree of acceptability of revisionary action, when that action may occur (1) in a summary other than of genetically related populations (as for example in regional lists), or (2) in advance of attainment of all knowledge that may have a bearing upon the restriction, or (3) without direct examination, with all pertinent facts in mind, of the onomatophore.

5. Recommendation is made that any considered revision of type locality should be regarded as valid until proved wrong.

23. Type Genera in Synonymy and Their Relationship to Stability in Family Names. James A. Slater and Jean L. Laffoon, Iowa State University.

The authors feel that Articles 4 and 5 of the International Rules have been misin-

terpreted in the past. It is felt that the basis exists under the present rules for retaining family names based upon type genera subsequently placed in synonymy.

The practice of retaining family names based upon type genera in synonymy is considered desirable in view of the resultant increase in stability of family names.

Consideration of this subject by the Nomenclature Committee of the Society of Systematic Zoology is requested.

24. Plea for a Uniform Ending for All Order Names in Zoology. Harley P. Brown, University of Oklahoma.

To anyone dealing with the taxonomy of many animal phyla, the need for uniform ordinal names is apparent. The ending employed by ornithologists (*-iformes*) appears to be quite satisfactory. I recommend that a committee be established by the Society of Systematic Zoology, the functions of which committee would be as follows: (1) to consider the choice of an ending or suffix, (2) to apply the suffix selected to all accepted order names, ascertaining that there are no duplicates, (3) to outline rules of application of the suffix to any order names which may be proposed hereafter, and (4) to present a printed or mimeographed report of the above-listed matters at the next annual meeting of the Society of Systematic Zoology. If, after consideration and discussion of the committee's proposals, the membership of the society should approve them, proper steps might then be taken through the International Commission on Zoological Nomenclature.

25. Seasonal and Geographical Variation in North American Daphnia. John L. Brooks, Yale University.

The adult morphology of most species of *Daphnia* is seasonally variable. Both the pattern and extent of this seasonal variation within any species show geographical differences. Some of the geographical distribution patterns are illustrated.

26. Notes on a Collection of Chinese, Japanese, and Korean Myriapoda. Yu-Hsi M. Wang, University of Utah.

(No abstract received.)

27. Distribution of Pseudoscorpions in North-central New Mexico. C. Clayton Hoff, University of New Mexico.

The preliminary report is concerned with the distribution of various species of pseudoscorpions in north-central New Mexico and will indicate the niches occupied by various species and their patterns of distribution with respect to biotic communities.

28. First Supplement to the Myriapoda of the Philippine Islands. Yu-Hsi M. Wang, University of Utah.

(No abstract received.)

MEMBERSHIP

So many new members have been admitted to the Society during 1951 and 1952 that the charter members are no longer a majority of the membership. The growth of the Society has been fairly regular and has not yet tapered off noticeably. Charter members are those who joined before the First Annual Meeting on September 12, 1948; they totalled 343. In 1949 the membership increased to 531. At this time the first goal of 1000 members was set, and greater efforts were used to add to the membership.

In 1950, 269 new members were enrolled, bringing the total to 800. In 1951 there was a net increase of 230, topping the goal of 1000. As this is written in early November, we have 1175 members, with new applications arriving at the rate of more than one a day. This will surely bring the total above 1200 by the time of the annual meeting.

ARREARS

It is a matter of much concern to the treasurer that members are not all prompt in paying their annual bills. This year, two months after the bills were mailed, one third of the members (about 350) had not responded. On November 1st there were still 200 unpaid, in spite of second and third notices.

Judging from previous years, most of these members will eventually pay up. In 1952 arrears from 1951 were paid by 80 members, who thereby reinstated themselves into actual membership. The loss of the $200 from these delinquent members is a serious one to our small treasury, and the late receipt of dues adds very greatly to the work of the secretary-treasurer and frequently leads to errors in mailing the journal, the news letters,

and other items. Each member in arrears for one year costs the Society in extra postage and stationery at least $.25, which is a total loss so far as the activities of the Society are concerned.

FIFTH ANNUAL MEETING

By the time the reprints of this number of the journal are distributed, the Fifth Annual Meeting will be over. Every member will have received a complete outline of the program, which is an elaborate one, very carefully planned. A detailed report of the meeting will appear in the first number of Volume 2 of this journal.

SUPPLEMENTAL LIST OF MEMBERS

This is the third supplement to the complete list of members issued in December 1951. The first and second supplements appeared in SYSTEMATIC ZOOLOGY Volume 1, Number 1 and Number 2, respectively.

ABBOTT, R. TUCKER. 1952. Div. of Mollusks, U.S. National Museum, Washington 25, D. C. (Mollusks)

ANDERSON, MARLOWE G. 1952. P. O. Box 97, State College, N. Mex. (Trematoda, Cestoda)

BARGREN, WILLIAM C. 1952. 1664 Hadlock Street S.W., Atlanta, Ga. (Culicidae: Diptera)

BARR, A. R. 1952. Div. of Entomology, University Farm, St. Paul 1, Minn. (Anthropods of medical importance)

BEEBE, WILLIAM. 1952. New York Zoological Society, New York 60, N. Y. (Neotropical vertebrates)

BHATIA, M. L. 1952. Zoology Dept., Delhi Univ., Delhi, India. (Hirudinea)

BOARDMAN, RICHARD S. 1952. (U.S. Geological Survey) Room 338, U.S. National Museum, Washington 25, D. C. (Paleozoic Bryozoa)

BOOKHOUT, G. G. 1952. Dept. of Zoology, Duke Univ., Durham, N. C. (Polychaeta, Cirrepedia)

BOULTON, RUDYERD. 1952. 3234 Reservoir Rd. N.W., Washington 7, D. C. (Birds)

BRECKENRIDGE, W. J. 1952. Minnesota Museum of Natural History, Univ. of Minnesota, Minneapolis 14, Minn. (Birds, mammals, reptiles, amphibians)

BUMZAHEM, C. B. 1952. 1929 N. Bissell St., Chicago 14, Ill. (Amphibians, reptiles)

BURCH, JOHN B. 1952. P. O. Box 141, Ashland, Va. (Mollusks, Protozoa)

CAMERON, THOMAS W. M. 1952. P. O. Box 231, Macdonald College, Prov. Quebec, Canada. (Parasitology)

CASO MUNOZ, SRITA. MARIA ELENA. 1952. Inst. de Biologia, Casa del Lago Chapultepec, Mexico, D. F., Mexico. (Fossil and recent echinoderms)

CHANDLER, LELAND. 1952. Entomology Dept., Purdue Univ., Lafayette, Ind. (Insects)

CLARK, EDWIN C. 1952. Univ. of California Experiment Station, Box 8, Albany, Calif.

CLAY, WILLIAM M. 1952. Dept. of Biology, Univ. of Louisville, Louisville 8, Ky. (Reptiles, amphibians)

CLENCH, HARRY K. 1952. Sect. of Insects and Spiders, Carnegie Museum, Pittsburgh 13, Pa. (Lepidoptera: Lycaenidae)

COHEN, DANIEL M. 1952. Natural History Museum, Stanford Univ., Calif. (Fishes)

COHER, EDWARD I. 1952. Fernald Hall, Univ. of Massachusetts, Amherst, Mass. (Diptera: Mycetophilidae, Culicidae, Diopsidae)

COHN, THEODORE J. 1952. 3987 Saxon Ave., New York 63, N. Y. (Orthoptera, Tettigoniidae)

COLBERT, EDWIN H. 1952. American Museum of Natural History, New York 24, N. Y. (Fossil reptiles and amphibians)

COOK, EDWIN F. 1952. Div. of Entomology, University Farm, St. Paul 1, Minn. (Diptera, Mallophaga, Anoplura)

COX, SAM M. 1952. 127 N. 10th Ave. East, Duluth 5, Minn. (Insect larvae)

CROCKER, DENTON W. 1952. Dept. of Biology, Amherst College, Amherst, Mass. (Crayfishes)

DAVIS, WM. K. 1952. Biology Dept., Southwest Texas State Teachers College, San Marcos, Tex. (Reptiles, amphibians)

DIETZ, CURT. 1952. Box 3039, Stanford Univ., Calif.

DILGER, WILLIAM C. 1952. Lab. of Ornithology, Fernow Hall, Cornell Univ., Ithaca, N. Y. (Birds)

DUGGAN, T. L. 1952. Biology Dept., Loyola Univ., New Orleans 18, La.

EDDY, SAMUEL. 1952. Dept. of Zoology, Univ. of Minnesota, Minneapolis 14, Minn. (Fishes, plankton)

EHRLICH, PAUL R. 1952. (Univ. of Pennsylvania) 538 Academy St., Maplewood, N. J. (Lepidoptera: Satyridae)

EMERSON, MAJOR K. C. 1952. CCB, 2nd A.D., c/o Postmaster, New York, N. Y. (Ectoparasites of birds and mammals)

ENGEL, DR. H. 1952. Zoologisch Museum, Plantage Middenlaan 53, Amsterdam C, Netherlands. (Echinoderms, nudibranchs, Hirudinea)

FEIO, JOSÉ LACERDA DE ARAUJO. 1952. Museu Nacional, Quinta da Boa Vista, Rio de Janeiro, Brasil. (Arachnida, pseudoscorpions)

FENNAH, R. G. 1952. Imperial College of Tropical Agriculture, St. Augustine, Trinidad, B. W. I. (Homoptera: fulguroids and cercopoids)

FISCHER, ROLAND L. 1952. Div. of Entomology, Univ. of Minnesota, St. Paul 1, Minn. (Hymenoptera: Apoidea)

FOOTE, RICHARD H. 1952. (U.S. Dept. of Agriculture) U.S. National Museum, Washington 25, D. C. (Diptera: Culicidae)

FOSTER, RICHARD W. 1952. Museum of Comp. Zoology, Cambridge 38, Mass. (Marine mollusks)

FOX, RICHARD M. 1952. Colorado College, Colorado Springs, Colo. (Lepidoptera: Ithomiidae; Diptera: Nycteribiidae; Mallophaga)

GATHMAN, CRAIG A. 1952. 1101 21st Ave. N., Lake Worth, Fla. (Fishes, marine biology)

GIBBS, ROBERT H., JR. 1952. 414 Stewart Ave., Ithaca, N. Y. (Fishes)

GRAHAM, JOSEPH J. 1952. School of Mineral Sciences, Stanford Univ., Calif. (Fossil Foraminifera)

HAHNERT, W. F. 1952. Dept. of Zoology, Ohio Wesleyan Univ., Delaware, Ohio. (Invertebrates)

HANCOCK, CAPT. ALLAN. 1952, Patron. Allan Hancock Foundation, Univ. of Southern California, Los Angeles 7, Calif.

HARGIS, W. J., JR. 1952. Dept. of Zoology, Florida State Univ., Tallahassee, Fla. (Trematoda, Monogenea, fishes)

HAYDEN, ELLIS B., JR. 1952. Dept. of Entomology, Univ. of Kansas, Lawrence, Kans. (Diptera: Tendipedidae; aquatic insects)

HERMAN, CARLTON M. 1952. Patuxent Research Refuge, Laurel, Md. (Protozoan blood parasites)

HOCK, RAYMOND J. 1952. U.S. Public Health Service, P. O. Box 960, Anchorage, Alaska. (Vertebrates)

HOUCK, WARREN J. 1952. Route 1, Bayside, Calif.

HUBBELL, THEODORE H. 1952. Museum of Zoology, Univ. of Michigan, Ann Arbor, Mich. (Orthoptera)

JONES, E. W. KNIGHT. 1952. Marine Biology Station, Univ. College of North Wales, Bangor, Wales, (Hirudinea: Piscicolidae; Enteropneusta)

KARDOS, ERVIN H. 1952 Dept. of Entomology, Univ. of Kansas, Lawrence, Kans. (Acarina: Trombiculidae)

KAY, MAIRÉ WEIR. 1952. G. & C. Merriam Co., 47 Federal St., Springfield 2, Mass.

LABERGE, W. E. 1952. Dept. of Entomology, Univ. of Kansas, Lawrence, Kans. (Hymenoptera: Apoidea, Formicidae)

LIDICKER, WILLIAM Z., JR. 1952. 116-17 Union Turnpike, Forest Hills, N. Y. (Vertebrates)

LOUKASHKIN, ANATOLE S. 1952. 1210 23rd Ave., San Francisco 22, Calif. (Mammals, birds)

LUDWIG, PAUL D. 1952. Dept. of Zoology and Entomology, Ohio State Univ., Columbus 10, Ohio. (Leafhoppers, spiders)

MACNAB, JAMES A. 1952. 3440 N.W. Thurman, Portland 10, Ore. (Oligochaetes)

MANN, K. H. 1952. Dept. of Zoology, The University, Reading, Berks, England. (Hirudinea)

MATTHEWS, L. HARRISON. 1952. Zoological Society of London, Regent's Park, London, N.W. 8, England.

MCLANE, WM. N. 1952 Box 87, Welaka, Fla. (Fishes)

MEHRING, ALBERT C. 1952. Dept. of Conservation, Fernow Hall, Cornell Univ., Ithaca, N. Y. (Fishes, insects)

MERRILL, ARTHUR S. 1952. Museum of Comparative Zoology, Cambridge 38, Mass. (Marine mollusks)

NEAL, G. M. 1952. Dept. of Zoology, Univ. of Toronto, Toronto 5, Ont., Canada. (Rotifera)

NORLAND, CALVERT E. 1952. San Diego State College, San Diego 5, Calif. (Economic insects)

PAHL, BRO. GEORGE. 1952. Box 184, Notre Dame, Ind. (Gastropoda, Amphibia)

PARFIN, MISS SOPHY I. 1952. Div. of Insects, U.S. National Museum, Washington 25, D. C.

PECK, OSWALD. 1952. Div. of Entomology, Science Service Bldg., Ottawa, Ont., Canada. (Hymenoptera: Chalcidoidea)

PHELPS, WILLIAM H. 1952. Apartado 2009, Caracas, Venezuela. (Birds)

POLL, MAX. 1952. Musee Royal du Congo Belge, Tervuren, Belgium. (Fishes)

POTTS, ROBERT W. L. 1952. Agriculture Bldg., Embarcadero at Mission, San Francisco 5, Calif. (Lepidoptera: Nymphalidae; Coleoptera: Scarabaeidae; Hymenoptera: Formicidae)

PRITCHARD, A. EARL. 1952. Div. of Entomology, Univ. of California, Berkeley 4, Calif.

PROBST, ROBERT T. 1952. Dept. of Zoology and Entomology, Iowa State College, Ames, Iowa. (Fish parasites, aquatic Hemiptera)

PULS, PROF. DR. J. J. 1952. Caixa Postal 989, Londrina, Parana, Brasil. (Insects)

RAMOS, J. A. 1952. Biology Dept., Univ. of Puerto Rico, Mayaguez, Puerto Rico. (Homoptera)

RATTENBURY, JOAN C. 1952. Dept. of Zoology, McGill Univ., Montreal, Que., Canada. (Phoronidea, Bryozoa, Endoprocta)

RIPLEY, S. DILLON. 1952. Peabody Museum of Natural History, Yale Univ., New Haven, Conn. (Birds)

ROBERTSON, WILLIAM B., JR. 1952. Everglades National Park, Box 275, Homestead, Fla. (Birds, reptiles, amphibians)

SACHTLEBEN, PROF. DR. HANS. 1952. Deutsches Ent. Inst., Waldowstrasse 1, Berlin-Friedrichshagen, Germany. (Parasitic Hymenoptera)

SACKS, MARTIN. 1952. 1010 W. Green, Urbana, Ill. (Gastrotricha, Trematoda)

SANDERS, OTTYS. 1952. P.O. Box 4084, Station A, Dallas 8, Tex. (Amphibia)

SAYLOR, LAWRENCE W. 1952. State Health Dept., Room 300 Essex Bldg., Norfolk 10, Va. (Coleoptera: Scarabaeidae; ticks)

SCHENCK, HUBERT G. 1952. Box 1528, Stanford Univ., Calif. (Micro-fossils)

SCHICK, ROBERT X. 1952. (Univ. of California at Los Angeles) 1216 Sunset Blvd., Los Angeles 26, Calif. (Arachnida, Insecta)

SCOTT, FREDERIC R. 1952. 27 Malvern Ave., Apt. 2, Richmond 21, Va. (Birds)

SHAW, CHARLES E. 1952. 1025 Johnson Ave., San Diego 3, Calif. (Reptiles)

SHAW, F. R. 1952. Fernald Hall, Univ. of Massachusetts, Amherst, Mass. (Diptera: Mycetophilidae; honey bees)

SHINNERS, LLOYD H. 1952. Box 612, Southern Methodist Univ., Dallas 5, Texas. (Hymenoptera)

SMITH, CLARENCE LAVETT, JR. 1952. Museum of Zoology, Univ. of Michigan, Ann Arbor, Mich. (Fresh-water fishes)

SNYDER, D. P. 1952. Carnegie Museum, Pittsburgh 13, Pa. (Mammals)

SOGANDARES, B. FRANKLIN. 1952. 924 Lowerline St., New Orleans 18, La. (Mammals, parasites)

SOLJAN, DR. TONKO. 1952. Inst. Oceanographique Rib. FNRJ, Split, Yugoslavia.

STEINITZ, H. 1952. Dept. of Anatomy, Yale School of Medicine, New Haven 11, Conn. (Fishes, amphibians)

STIRTON, R. A. 1928. Dept. of Paleontology, Univ. of California, Berkeley 4, Calif. (Recent and fossil mammals)

SUZUKI, HOWARD K. 1952. Dept. of Zoology, Tulane Univ., New Orleans 18, La. (Amphibians, reptiles)

THATCHER, T. O. 1952. Dept. of Entomology, Colorado A. & M. College, Fort Collins, Colo. (Coleoptera: Scolytidae)

VOLPE, E. PETER. 1952. Dept. of Biology, Newcomb College, Tulane Univ., New Orleans 18, La. (Amphibia)

WARNER, MISS ROSE ELLA. 1952. (U.S. Dept. of Agriculture) Room 394, U.S. National Museum, Washington 25, D. C. (Coleoptera: Curculionidae)

WESENBERG-LUND, ELISE. 1952. Zoological Museum, Krystalgade, Copenhagen, Denmark. (Polychaetes, brachiopods)

CHANGES OF ADDRESS

The following changes of address have been recorded since the previous supplement.

ABRAMOFF, PETER. 1951. Dept. of Zoology, Univ. of Wisconsin, Madison 6, Wis. (Mammalogy, limnology)

ALVAREZ, DR. JOSÉ. 1950. Calle 27, Num. 71, S. Pedro de los Pinos, D. F., Mexico. (Fresh-water fishes)

BERBERIAN, J. F. 1949. Dept. of Biological Sciences, Duquesne Univ., Pittsburgh 19, Pa. (Comparative morphology and paleontology)

BLACK, JOHN D. (Suspended 1952)

BOUCHARD, J. LOUIS. 1948, C. Dept. of Biology, Northwestern State College, Alva, Okla. (Helminths)

BRANDT, B. B. 1948, C. Dept. of Zoology, North Carolina State College, Raleigh, N. C. (Herpetology, ichthyology)

BRATTSTROM, BAYARD H. 1951. Dept. of Zoology, Univ. of California, Los Angeles 24, Calif. (Reptiles)

BRIGGS, JOHN CARMON. 1952. Natural History Museum, Stanford Univ., Calif.

BROOKS, HAROLD K. 1950. Dept. of Geology and Geography, Univ. of Tennessee, Knoxville, Tenn. (Malacostraca)

CAMIN, JOSEPH H. 1951. Chicago Academy of Sciences, 2001 N. Clark St., Chicago 14, Ill. (Acarina, Reptilia)

CHITWOOD, B. G. 1951. Box 481, Marquette, Mich. (Nematodes, tardigrades, echinoderms, gastrotrichs, rotifers)

CLOTHIER, RONALD R. 1950. Dept. of Biology, Kansas Wesleyan Univ., Salina, Kans. (Insectivores)

COATS, RUTH E. 1950. 3846 Skyline Road, Carlsbad, Calif. (Fresh-water mollusks)

CURTIN, CHARLES B. 1951. 714 N. Euclid Ave., Pittsburgh 6, Pa. (Tardigrades, parasitic worms)

DIRKS, CHARLES O. (Resigned January 1, 1952)

DOETSCHMAN, WILLIS H. 1948, C. 994 Billings Blvd., San Leandro, Calif. (Mites of birds and mammals)

DOUTT, RICHARD L. 1951. P. O. Box 8, Albany, Calif. (Hymenoptera: Chalcidoidea)

DRAKE, ROBERT J. 1949. Lab. of Conchology, Biblioteca y Museo de Sonora, Hermosillo, Sonora, Mexico. (Mollusca)

EASTWOOD, JOHN P. 1950. Woodford, Wis. (Diptera: Calliphoridae)

EDGREN, RCHARD A. 1950. Div. of Biological Research, G. D. Searle & Co., P. O. Box 5110, Chicago 80, Ill. (Reptiles, amphibians)

EDMUNDS, GEORGE F., JR. 1948, C. Div. of Biology, Univ. of Utah, Salt Lake City 1, Utah. (Ephemeroptera)

EVANS, HOWARD E. 1948, C. Dept. of Entomology, Cornell Univ., Ithaca, N. Y. (Hymenoptera: Pompilidae)

FIELD, MRS. LOUISE RANDALL. 1950. 112 N. Holly Drive, Baytown, Tex. (Anthozoa)

FINKS, ROBERT M. 1952. 26 Clinton St., Apt. 2, New York 2, N. Y. (Tetracoralla, fossil Porifera)

FIRSCHEIN, I. LESTER. 1949. Biology Dept., New York Univ., New York 3, N. Y. (Herpetology)

FRANZEN, DOROTHEA. 1948, C. Illinois Wesleyan Univ., Bloomington, Ill. (Mollusca)

GORDON, MALCOLM S. 1951. 1305 53rd St., Brooklyn 19, N. Y. (Fishes, birds, mammals)

GREENFIELD, LEONARD J. 1950. Box 488, Univ. of Miami, Miami 46, Fla. (Brachyura, Copepoda)

GROSS, ALFRED O. (Resigned January 1, 1952)

HAGEN, KENNETH S. 1951. Box 8, Albany 6, Calif. (Hymenoptera: Serphoidea; Coleoptera: Anthicidae)

HARRY, HAROLD W. 1948, C. Academy of Natural Sciences, Philadelphia 3, Pa. (Mollusca)

HARRY, ROBERT R. 1948, C. Academy of Natural Sciences, Philadelphia 3, Pa. (Fishes)

HAZELTINE, WILLIAM E. 1950. 911 Lincoln Ave., Richmond 5, Calif. (Coleoptera: Scarabaeidae)

HOWARD, WALTER E. (Resigned January 1, 1952)

ILLG, PAUL L. 1948, C. Dept. of Zoology, Univ. of Washington, Seattle 5, Wash. (Copepoda)

JACKSON, C. F. 1948, C. Dept. of Zoology, Univ. of New Hampshire, Durham, N. H. (Vertebrates)

KAGAN, IRVING G. 1950. Zoological Laboratory, Univ. of Pennsylvania, Philadelphia 4, Pa. (Trematodes)

KLAK, GEORGE E. 1950. BOQ, Room 146, Treasure Island, San Francisco, Calif. (Plankton, limnology)

KNIPPING, PAUL. 1950. 597 Hazel St., Oshkosh, Wis. (Acarina, Siphonaptera)

KUITERT, LOUIS C. 1952. Agricultural Experiment Sta., Univ. of Florida, Gainesville, Fla.

DE LAUBENFELS, M. W. 1948, C. Dept. of Zoology, Oregon State College, Corvallis, Ore. (Porifera)

LEIGH, W. HENRY. 1950. Box. 488, Univ. of Miami, Miami 46, Fla. (Helminths)

LINSDALE, JEAN M. 1948, C. Jamesburg Route, Robles del Rio, Calif. (Vertebrates)

MACGINITIE, G. E. 1948 442 El Modena Ave., Newport Beach, Calif. (Marine invertebrates)

MARTIN, JOHN C. 1950. Dominion Parasite Lab., Box 179, Belleville, Ont., Canada. (Hymenoptera: Braconidae; Odonata)

MATTLIN, ROBERT H. 1951. Box 2337, Salt Lake City, Utah. (Reptiles)

MATTOX, N. T. 1948, C. Allan Hancock Foundation, Univ. of Southern California, Los Angeles 7, Calif. (Conchostraca, Mollusca)

MCMULLEN, DONALD B. 1951. Army Medical Service Graduate School, Walter Reed Medical Center, Washington 12, D. C. (Helminths)

MEYERS, ERNEST G. 1950. 1566 Harrison Ave., Fresno, Calif. (Diptera)

MILLER, E. MORTON. 1948, C. Box 488, Univ. of Miami, Miami 46, Fla. (Termites)

MOORE, J. EDWARD 1950. Dept. of Zoology, Univ. of Alberta, Edmonton, Alb., Canada. (Amphibians, reptiles, mammals)

MOORE, J. PERCY. 1948, C. Highland Ave., Route 1, Media, Pa. (Annelids, vertebrates)

NOLAND, GEORGE B. 1951. 908-B Walnut Lane, East Lansing, Mich. (Spiders, insects)

NOSOW, EDMUND. 1952. Box 21, Campus Sta., Univ. of Cincinnati, Cincinnati 21, Ohio. (Bryozoa)

PAPPAS, GEORGE DEMETRIOS. 1950. Apt. 6-B, 2440 Bronx Park East, New York 67, N. Y. (Protozoa, invertebrates)

PETERS, JAMES A. 1950. Dept. of Biology, Brown Univ., Providence 12, R. I. (Reptiles, amphibians)

PETTIBONE, MARIAN H. 1948, C. 4842 6th Ave., Tacoma 6, Wash. (Polychaetes)

QUAY, T. L. (Resigned January 1, 1952)

RAPP, WILLIAM F., JR. 1948, C. 2759 F St., Lincoln, Neb. (Diptera, Psychodidae)

REINTHAL, W. J. 1950. Terrell State Hospital, Terrell, Tex. (Lepidoptera)

RINGUELET, DR. RAUL A. 1948, C. Museo La Plata, Ciudad Eva Peron, Argentina. (Crustacea, Hirudinea)

RICHTER, PAUL O. 1950. Dept. of Entomology, Oregon State College, Corvallis, Ore. (Coleoptera, Scarabaeidae)

RIVAS, LUIS RENE. 1950. Box 488, Univ. of Miami, Miami 46, Fla. (Fishes)

ROECKER, ROBERT M. (Resigned April 25, 1952)

RUICK, J. D., JR. 1950. Apt. 2, 936 N.W. Second St., Miami, Fla. (Arachnida, Reptilia: Crotalidae)

RUSSELL, RICHARD W. 1950. 1243 Evelyn St., Berkeley, Calif. (Collembola, reptiles, amphibians)

RYAN, R. MARK. 1951. 9433 Brighton Way, Beverly Hills, Calif. (Mammals)

SCHILLER, EVERETT L. 1949. Arctic Health Research Foundation, Box 960, Anchorage, Alaska. (Cestodes)

SCHUURMANS STEKHOVEN, DR. J. H. 1950. Biological Laboratory, Noury en van der Lande, Ankersmitlaan 1, Deventer, Netherlands. (Nematodes, Diptera, copepods, isopods)

SCOTT, DONALD C. 1949. Dept. of Biology, Univ. of Georgia, Athens, Ga. (Fishes, aquatic insects)

SEDMAN, Y. 1952. King Hall, Dept. of Entomology, Univ. of Wisconsin, Madison 6, Wis. (Diptera)

SINCLAIR, G. WINSTON. 1950. Dept. of Geology and Paleontology, Ohio Wesleyan Univ., Delaware, Ohio. (Fossil invertebrates)

SMITH, F. G. WALTON. 1948, C. Box 488, Univ. of Miami, Miami 46, Fla. (Marine invertebrates)

SPILMAN, T. J. 1950. Dept. of Entomology, Cornell Univ., Ithaca, N. Y. (Coleoptera)

STEVENS, BELLE A. 1948, C. 4520 8th St. N.E., Seattle 5, Wash. (Crustacea)

STROHECKER, H. F. 1950. Box 488, Univ. of Miami, Miami 46, Fla. (Orthoptera; Coleoptera: Endomychidae)

SWAN, EMERY F. 1949. Dept. of Zoology, Univ. of New Hampshire, Durham, N. H. (Mollusca, serpulid worms)

TELLEZ, MRS. CLEMENCIA. 1950. Calle 27, Num. 71, S. Pedro de los Pinos, D. F., Mexico. (Collembola)

TINER, JACK D. 1948, C. Merck Institute, Rahway, N. J. (Nematodes)

TODD, GEORGE K. 1948, C. 809 W. 25th St., Cheyenne, Wyo. (Coleoptera)

TOWNES, HENRY K. 1948, C. MSA-STEM, APO 928, Postmaster, San Francisco, Calif. (Insects)

TUTTLE, DONALD M. 1948, C. Univ. of Arizona Farm, Route 1, Box 844, Yuma, Ariz. (Coleoptera)

VARGAS, DR. LUIS. 1948, C. Apartado Postal 19205, Mexico, D. F., Mexico. (Diptera)

VELARDO, J. T. (Resigned April 25, 1952)

VOSS, GILBERT L. 1950. Box 488, Univ. of Miami, Miami 46, Fla. (Cephalopods, marine mollusks)

WALTER, WALDEMAR M. 1952. College of Charleston, Charleston 10, S. C. (Mollusca, invertebrates)

WILLIAMS, JOSEPH L. 1948, C. 5518 W. Girard Ave., Philadelphia 31, Pa. (Lepidoptera, Coleoptera)

WILSON, MRS. MILDRED S. 1948, C. Box 960, Anchorage, Alaska. (Copepoda)

WING, MERLE W. 1948, C. Box 5425, Raleigh, N. C. (Ants)

WINSTON, PAUL W. 1950. Dept. of Biology, Univ. of Colorado, Boulder, Colo. (Coleoptera: Carabidae)

WOODIN, WILLIAM H. 1950. Route 2, Box 641, Tucson, Ariz. (Reptiles)

YOCHELSON, ELLIS. 1950. 1431 Somerset Place, N. W., Washington 11, D. C. (Paleozoic gastropods)

SYSTEMATIC ZOOLOGY is published quarterly by the Society of Systematic Zoology. Its purpose is threefold: To publish, and therefore to encourage the preparation of, contributions on basic aspects of all fields of systematics, principles and problems; to provide a suitable forum for discussion of the problems of the systematist and his methods; and to report as news the other activities of the Society of Systematic Zoology.

Contributions of the following types are solicited: Papers on principles and the applications of principles of wide implication and general interest in any phase of systematics, such as comparative anatomy, zoogeography, paleontology, taxonomy, classification, evolution, or genetics; discussions of methods, specific problems, and activities of systematists; discussions of new books and monographs; and news of systematists, organizations interested in systematics, research and teaching programs, expeditions, collections, meetings, and anything else of interest to systematists.

Major articles should be from 1500 to 10,000 words in length. Discussion articles should not be over 400 words unless of very general interest. Contributors are encouraged to submit line drawings and diagrams to illustrate their articles. Half-tones may be accepted where necessary to the article.

THE PUBLICATION FUND

A Publication Fund was established to help in the initiation of this journal. It consists entirely of donations from the members. It is hoped that it will be a permanent fund for use in other publication programs as well. For this reason further contributions of any amount will be welcomed. At present this fund is restricted to use for SYSTEMATIC ZOOLOGY, under the supervision of the Council.